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A SOURCE BOOK IN ASTRONOMY

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GREGORY D. WALCOTT · *General Editor*

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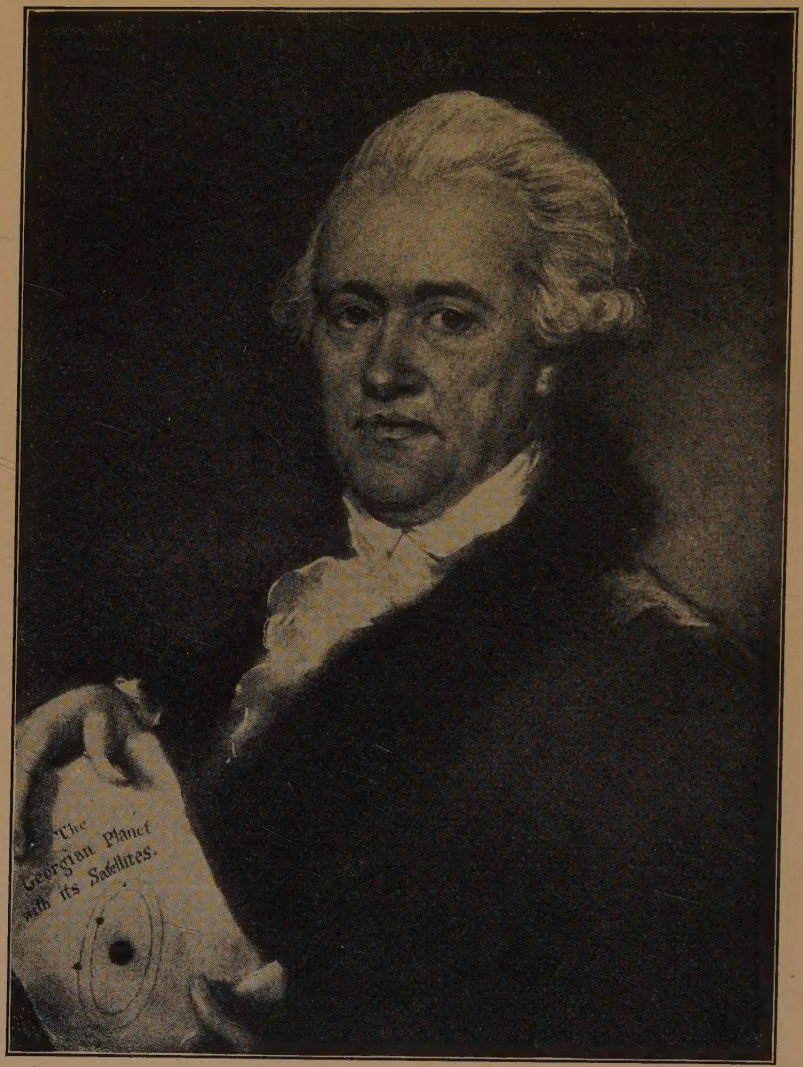

Wm. Herschel

From a pastel by J. Russell, R.A. 1794
In the possession of Sir W. J. Herschel Bart.

Frontispiece

A SOURCE BOOK
in
ASTRONOMY

By

HARLOW SHAPLEY, PH.D., LL.D.

Professor of Astronomy in Harvard University and Director of the Harvard Observatory

AND

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SOURCE BOOKS IN THE HISTORY OF THE SCIENCES

General Editor's Preface

THIS series of Source Books aims to present the most significant passages from the works of the most important contributors to the major sciences during the last three or four centuries. So much material has accumulated that a demand for selected sources has arisen in several fields. Source books in philosophy have been in use for nearly a quarter of a century, and history, economics, ethics, and sociology utilize carefully selected source material. Recently, too, such works have appeared in the fields of psychology and eugenics. It is the purpose of this series, therefore, to deal in a similar way with the leading physical and biological sciences.

The general plan is for each volume to present a treatment of a particular science with as much finality of scholarship as possible from the Renaissance to the end of the nineteenth century. In all, it is expected that the series will consist of eight or ten volumes, which will appear as rapidly as may be consistent with sound scholarship.

In June, 1924, the General Editor began to organize the following Advisory Board:

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Each of the scientists on this board, in addition to acting in a general advisory capacity, is chairman of a committee of four or five men, whose business it is to make a survey of their special field and to determine the number of volumes required and the contents of each volume.

In December, 1925, the General Editor presented the project to the Eastern Division of the American Philosophical Association. After some discussion by the Executive Committee, it was approved and the philosophers of the board, with the General Editor as chairman, were appointed a committee to have charge of it. In November, 1927, the Carnegie Corporation of New York granted $10,000 to the American Philosophical Association as a revolving fund to help finance the series. In December, 1927, the American Association for the Advancement of Science approved the project, and appointed the General Editor and Professors Edwin G. Conklin and Harlow Shapley a committee to represent that Association in cooperation with the Advisory Board. In February, 1928, the History of Science Society officially endorsed the enterprise. Endorsements have also been given by the American Anthropological Association, the Mathematical Association of America, the American Mathematical Society, and the American Astronomical Society within their respective fields.

The General Editor wishes to thank the members of the Advisory Board for their assistance in launching this undertaking; Dr. J. McKeen Cattell for helpful advice in the early days of the project and later; Dr. William S. Learned for many valuable suggestions; the several societies and associations that have given their endorsements; and the Carnegie Corporation for the necessary initial financial assistance.

GREGORY D. WALCOTT,
GENERAL EDITOR.

LONG ISLAND UNIVERSITY,
BROOKLYN, N. Y.
December, 1928.

A SOURCE BOOK IN ASTRONOMY

Authors' Preface

To select and prepare for publication the more important contributions in astronomy is not a simple matter. Several of the astronomical classics are not effectively quotable, because of the mathematical nature of the work, or the unwieldy length of a suitable excerpt, or the manner of writing the significant passage, which frequently depends too much on earlier papers or the preceding chapters of a book. We believe we have succeeded, however, in giving a fairly comprehensive synopsis of the great contributions to astronomy of the past four hundred years. Many of the quotations are essentially complete articles. The brevity with which some of the most striking discoveries have been announced and described has permitted the inclusion of more than sixty contributors. Many of these are the obviously great, of the stature of Copernicus, Newton, Laplace, and Herschel; others are quite unknown to the general reader, some of them even to the astronomer, but in our opinion their contributions are an essential part of the astronomical panorama.

With the comfort of the non-astronomical reader in mind, we have avoided heavy technicalities as much as possible, and except in such instances as the classical work of Gauss on the Method of Least Squares, we have evaded mathematical complications.

It is surprising how innocent the present-day astronomer is of first-hand knowledge of the classics in his science. Probably few have ever read original writings by Copernicus, Tycho Brahe, Horrox, Lambert, Gauss, and a dozen other leaders represented in the present volume. Great contributions of the past are known mainly through textbooks which in turn depend on older texts and histories. Such secondary information is more accessible and easier to understand; but our selections will show that many of the

important discoveries, interpretations, and theories of the past have been lucidly and beautifully set forth.

Although the Source Book in Astronomy is not to be considered a systematic and connected history of the subject, it should serve as a guide to the historical reader. The order is essentially chronological. We have made no attempt to relate one man's work with another's, and only occasionally have we tried to express the current evaluation of theories that at one time were more significant than now. The brief biographical notes merely identify the time, country, and major activities of the various scientists concerned.

Contributions originating since 1900 have not been considered, nor are the discoveries of living astronomers taken into account. The increased activity in astronomy, especially in America, towards the end of the nineteenth century, has resulted in a large number of contributions during the interval from 1850 to 1900. Even with this emphasis on recent astronomical work we should make no claim that the present development of the science is fully pictured by our survey. Sidereal astronomy in particular has been revolutionized since the close of the century; nevertheless, nearly all the discoveries and theories described in this volume stand firm, in demonstration, perhaps, of the essential safety of interpretation based on mathematical principles and the careful observation of physical phenomena.

It should be noted that the spelling, punctuation, and typographical style of the originals have been retained in nearly all instances, notwithstanding the frequent inconsistencies that result. Our own comments, whether in the text or footnotes, have been enclosed in square brackets. In the investigation and selection of material, and in all parts of the preparation of the volume, we have shared equally. Miss Agnes Hoovens worked for several months on editorial details. Mrs. Harlow Shapley and Dr. John H. Walden have helped extensively in the translations.

To several of our colleagues at the Harvard Observatory, especially to Dr. W. J. Fisher and to Professor B. P. Gerasimovič, we are indebted for advice and assistance. We have received valuable suggestions from Professor John C. Duncan of Wellesley College, Professor W. Carl Rufus of the University of Michigan, Professor Frank Schlesinger of Yale University, Professor Harlan T. Stetson of Harvard University, and Professor Anne S. Young of Mount Holyoke College. We are indebted to Houghton Mifflin Company

for the quotations from Percival Lowell's "Mars;" to the American Book Company for the excerpts from J. S. Ames' translation of Fraunhofer's "Prismatic and Diffraction Spectra;" to Longmans, Green, and Co. for the quotations from Helmholtz' "Popular Lectures on Scientific Subjects;" and to D. Appleton and Company for the passages from the sixth edition of Spencer's "First Principles." To the Council of the American Astronomical Society we desire to express our appreciation of its generous endorsement of the plan to prepare a Source Book in Astronomy.

HARLOW SHAPLEY.
HELEN E. HOWARTH.

CAMBRIDGE, MASSACHUSETTS,
December, 1928.

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A SOURCE BOOK IN ASTRONOMY

COPERNICUS[1]

A Theory that the Earth Moves around the Sun

(From "De Revolutionibus Orbium Celestium," 1543.)

Chapter I. That the Universe is Spherical

First of all we assert that the universe is spherical; partly because this form, being a complete whole, needing no joints, is the most perfect of all; partly because it constitutes the most spacious form, which is thus best suited to contain and retain all things; or also because all discrete parts of the world, I mean the sun, the moon and the planets, appear as spheres; or because all things tend to assume the spherical shape, a fact which appears in a drop of water and in other fluid bodies when they seek of their own accord to limit themselves. Therefore no one will doubt that this form is natural for the heavenly bodies.

Chapter II. That the Earth is Likewise Spherical

That the earth is likewise spherical is beyond doubt, because it presses from all sides to its center. Although a perfect sphere is not immediately recognized because of the great height of the mountains and the depression of the valleys, yet this in no wise invalidates the general spherical form of the earth. This becomes clear in the following manner: To people who travel from any place to the North, the north pole of the daily revolution rises gradually, while the south pole sinks a like amount. Most of the stars in the neighborhood of the Great Bear appear not to set, and in the South some stars appear no longer to rise. Thus Italy does

[1] Nicholas Copernicus (1473–1543), Polish astronomer, in "De Revolutionibus Orbium Celestium" established the heliocentric view of the planetary system, which slowly but surely revolutionized the science of astronomy.

not see Canopus, which is visible to the Egyptians. And Italy sees the outermost star of the River, which is unknown to us of a colder zone. On the other hand, to people who travel toward the South, these stars rise higher in the heavens, while those stars which are higher to us become lower. Therefore, it is plain that the earth is included between the poles and is spherical. Let us add that the inhabitants of the East do not see the solar and lunar eclipses that occur in the evening, and people who live in the West do not see eclipses that occur in the morning, while those living in between see the former later, and the latter earlier.

That even the water has the same shape is observed on ships, in that the land which can not be seen from the ship can be spied from the tip of the mast. And, conversely, when a light is put on the tip of the mast, it appears to observers on land gradually to drop as the ship recedes until the light disappears, seeming to sink in the water. It is clear that the water, too, in accordance with its fluid nature, is drawn downwards, just as is the earth, and its level at the shore is no higher than its convexity allows. The land therefore projects everywhere only as far above the ocean as the land accidentally happens to be higher . . .

Chapter IV. That the Motions of the Heavenly Bodies Are Uniform, Circular, Uninterrupted, or Are Made Up of Combined Circular Motions

Hereupon, we note that the motions of the heavenly bodies are circular. When a sphere is in motion it rotates, expressing, through this activity, its form as that of the simplest of bodies, in which there is to be found neither a beginning nor an end; nor can the beginning be distinguished from the end, as the sphere achieves, through the same intermediate points, its original position. Because of the multiplicity of circles there are, however, numerous possible motions. The best known of all is the daily revolution which the Greeks call Nychthemeron, *i.e.*, the period of day and night. To achieve this motion, it is believed, the whole universe with the exception of the earth, turns from east to west. It is recognized as the common measure of all motions, since time itself is measured chiefly by the number of days. In addition, we see progressing other revolutions which are apparently retrograde, *i.e.*, from west to east; namely those of the sun, the moon, and the five planets.

By means of this motion the sun measures for us the year, the moon the month, as the most common units of time. And thus each of the other five planets completes its orbit. Yet they are peculiar in many ways. First, in that they do not revolve about the same poles around which the first motion takes place, progressing instead in the oblique path of the Zodiac; second, in that they do not seem to move uniformly in their own orbits, for the sun and the moon are discovered moving now with a slower, now a faster motion. The remaining five planets, moreover, we also see at times going backward and, in the transition, standing still. And while the sun moves along always in its direct path, the planets wander in various ways, roaming, now to the South, now to the North. Wherefore they are designated "*planets*." They have the added peculiarity that they at times come nearer to the earth, where they are called *at perigee*, then again they recede from it, where they are called *at apogee*. Nevertheless, it must be admitted that the motions are circular, or are built up of many circles; for thus such irregularities would occur according to a reliable law and a fixed period, which could not be the case if they were not circular. For the circle alone can bring back the past, as the sun, so to speak, brings back to us, through its motion made up of circles, the irregularities of the days and nights and the four seasons; in which several motions are recognized because it cannot happen that the simple heavenly bodies move irregularly in a single circle. For this would either have to be caused by an inconstancy in the nature of the moving force—whether the inconstancy be brought about by a cause from without or within—or would have to originate in an irregularity of the moving body. But as reason rebels against both, and as it is unworthy to assume such a thing concerning that which is arranged in the best of order, so one must admit that the regular motions seem irregular to us, either because the various circles have different poles, or because the earth is not situated in the center of the circles in which the planets move; and that to us who observe the motions of the stars from the earth, the planets, because of the varying distances, appear larger when near us than when they are in paths more remote; that can be proved in optics. In this way the motions which take place in equal times through equal arcs, seem to us unequal due to different distances. Therefore, I consider it above all things necessary that we investigate carefully what relation the earth has to the heavens, so that we, when we wish to

investigate the most noble things in nature, do not leave out of consideration the nearest, and erroneously attribute to the heavenly bodies what belongs to the earth.

Chapter V. Whether the Earth Has a Circular Motion, and Concerning the Location of the Earth

Since it has already been proved that the earth has the shape of a sphere, I insist that we must investigate whether from its form can be deduced a motion, and what place the earth occupies in the universe. Without this knowledge no certain computation can be made for the phenomena occurring in the heavens. To be sure, the great majority of writers agree that the earth is at rest in the center of the universe, so that they consider it unbelievable and even ridiculous to suppose the contrary. Yet, when one weighs the matter carefully, he will see that this question is not yet disposed of, and for that reason is by no means to be considered unimportant. Every change of position which is observed is due either to the motion of the observed object or of the observer, or to motions, naturally in different directions, of both; for when the observed object and the observer move in the same manner and in the same direction, then no motion is observed. Now the earth is the place from which we observe the revolution of the heavens and where it is displayed to our eyes. Therefore, if the earth should possess any motion, the latter would be noticeable in everything that is situated outside of it, but in the opposite direction, just as if everything were traveling past the earth. And of this nature is, above all, the daily revolution. For this motion seems to embrace the whole world, in fact, everything that is outside of the earth, with the single exception of the earth itself. But if one should admit that the heavens possess none of this motion, but that the earth rotates from west to east; and if one should consider this seriously with respect to the seeming rising and setting of the sun, of the moon and the stars; then one would find that it is actually true. Since the heavens which contain and retain all things are the common home of all things, it is not at once comprehensible why a motion is not rather ascribed to the thing contained than to the containing, to the located rather than to the locating. This opinion was actually held by the Pythagoreans Heraklid and Ekphantus and the Syracusean Nicetas (as told by Cicero), in that they assumed the earth to be rotating in

the center of the universe. They were indeed of the opinion that the stars set due to the intervening of the earth, and rose due to its receding.

From this assumption follows the other not less important doubt concerning the position of the earth, though it is assumed and believed by almost everyone that the earth occupies the center of the universe. If, therefore, one should maintain that the earth is not in the center of the universe, but that the discrepancy between the two is not great enough to be measurable on the sphere of the fixed stars, but on the other hand noticeable and recognizable in the orbits of the sun and the planets; and if further he were of the opinion that the motions of the latter for this reason appear irregular, just as if they were oriented with respect to another center than that of the earth—such a person might, perhaps, have assigned the true reason for the apparently irregular motions. For since the planets appear now nearer, now more distant from the earth, this betrays necessarily that the center of the earth is not the center of those circular orbits. And yet it is not determined whether the earth decreases and increases its distance from them or they their distance from the earth.

It would thus not be strange if someone should ascribe to the earth, in addition to its daily rotation, also another motion. However, it is said that the Pythagorean Philolaus, a not ordinary mathematician, believed that the earth rotates, that it moves along in space with various motions, and that it belongs to the planets; wherefore, Plato did not delay journeying to Italy to interview him, as is told by those who have described Plato's life. Many, on the other hand, believed that it could be proved by mathematical calculation that the earth is situated in the center of the universe, and since, compared with the enormous size of the heavens, it can be considered as a point, it occupies the central point and is for this reason immovable; because if the universe moves, its central point must remain motionless, and that which is nearest the central point must move most slowly.

Chapter VII. Why the Ancients Believed That the Earth Rests in the Middle of the Universe, as Its Central Point

Thus for certain other reasons the ancient philosophers sought to prove that the earth is in the center of the universe. As chief cause, however, they cite weight and imponderability. The element

earth, is, to be sure, the heaviest of all, and everything ponderable tends to move, governed by its impulse, toward the innermost center of the earth. Now since the earth is spherical—the earth, onto the surface of which heavy bodies from all sides fall perpendicularly, due to their own nature—the falling bodies would meet at its center if they were not held back on the surface; because, indeed, a straight line which is perpendicular to the tangent plane at its point of tangency leads to the center. As to those bodies which move toward the center, it seems to follow that they would come to rest at the center. All the more would the whole earth be at rest in the center, and no matter what it might accumulate in the way of falling bodies, it would remain motionless due to its own weight.

In a similar manner the ancients support their proofs with the cause of motion and its nature. Aristotle says, for example, that a simple body has a simple motion; of possible simple motions, however, one is motion in a straight line, the other is circular motion. Of simple motions in a straight line, one is upwards, and the other is downwards. Therefore, every simple motion would be either toward the center, *i.e.* downward, or away from the center, *i.e.* upwards, or around the center, and this would be the circular motion or revolution. Only the earth and the water, which are considered heavy, move downwards, that is, tend to move towards the center. Air, however, and fire, which are endowed with imponderability, move upwards and away from the center. It seems clear that one must admit motion in a straight line for these four elements; as regards the heavenly bodies, however, one must admit motion in a circle around the center. Thus says Aristotle.

"If, therefore," says the Alexandrian Ptolemy, "the earth turns, at least in daily rotation, the opposite of all that is said above must take place; that is to say the motion which traverses throughout the whole circumference of the earth in twenty-four hours would have to be the most violent of all and its velocity would have to be transcendent. But matter which is set in violent rotation does not seem at all fit to be massed together, but rather to be dispersed, if the component parts are not held together with some firmness. And long before now, he says, the disintegrated earth would have been dissipated over the heavens themselves, which is very ridiculous; and much less would the living beings and other separated masses in any way have remained unannihilated. But also the bodies falling in straight lines would not arrive on the places

destined for them, as these spots would in the meantime have moved from under with such great velocity. We would also see the clouds and whatever else is floating in the air always moving toward the west."

Chapter VIII. Refutation of the Arguments, and Their Insufficiency

From these and similar reasons it is claimed that the earth is at rest in the center of the universe and that this is undoubtedly true. But one who believes that the earth rotates will also certainly be of the opinion that this motion is natural and not violent. Whatever is in accordance with nature produces effects which are the opposite of what happens through violence. Things upon which violence or an external force is exerted must become annihilated and cannot long exist. But whatever happens in the course of nature remains in good condition and in its best arrangement. Without cause, therefore, Ptolemy feared that the earth and all earthly things if set in rotation would be dissolved by the action of nature, for the functioning of nature is something entirely different from artifice, or from that which could be contrived by the human mind. But why did he not fear the same, and indeed in much higher degree, for the universe, whose motion would have to be as much more rapid as the heavens are larger than the earth? Or have the heavens become infinite just because they have been removed from the center by the inexpressible force of the motion; while otherwise, if they were at rest, they would collapse? Certainly if this argument were true the extent of the heavens would become infinite. For the more they were driven aloft by the outward impulse of the motion, the more rapid would the motion become because of the ever increasing circle which it would have to describe in the space of 24 hours; and, conversely, if the motion increased, the immensity of the heavens would also increase. Thus velocity would augment size into infinity, and size, velocity. But according to the physical law that the infinite can neither be traversed, nor can it for any reason have motion, the heavens would, however, of necessity be at rest.

But it is said that outside of the heavens there is no body, nor place, nor empty space, in fact, that nothing at all exists, and that, therefore, there is no space in which the heavens could expand; then it is really strange that something could be enclosed by nothing. If, however, the heavens were infinite and were bounded

C. Plinius, Sec. Lib. 2. c. 10.
150.
IMPROBVM AVSVM
VERVM ITA SVBTILI ARGVMENTATIONE COMPREHENSVM
VT PVDEAT NON REDERE
Maculæ et Faculæ ex uariis obſeruandi modis ſtabiliuntur.

Early astronomical instruments. From the "Rosa Ursina" (1630) of Christopher Scheiner (1575–1650), German Jesuit, whose famous book dealt mainly with sunspots, faculæ, and other solar phenomena.

only by their inner concavity, then we have, perhaps, even better confirmation that there is nothing outside of the heavens, because everything, whatever its size, is within them; but then the heavens would remain motionless. The most important argument, on which depends the proof of the finiteness of the universe, is motion. Now, whether the world is finite or infinite, we will leave to the quarrels of the natural philosophers; for us remains the certainty that the earth, contained between poles, is bounded by a spherical surface. Why should we hesitate to grant it a motion, natural and corresponding to its form; rather than assume that the whole world, whose boundary is not known and cannot be known, moves? And why are we not willing to acknowledge that the *appearance* of a daily revolution belongs to the heavens, its *actuality* to the earth? The relation is similar to that of which Virgil's Æneas says: "We sail out of the harbor, and the countries and cities recede." For when a ship is sailing along quietly, everything which is outside of it will appear to those on board to have a motion corresponding to the movement of the ship, and the voyagers are of the erroneous opinion that they with all that they have with them are at rest. This can without doubt also apply to the motion of the earth, and it may appear as if the whole universe were revolving.

Now what shall we say about the clouds and whatever else is somehow floating, falling or rising in the air? Except that not only does the earth move with its attached watery element, but it also carries with it no small part of the air and whatever else is thus joined with the earth. It may be that the air lying nearest the earth, mixed with earthy and watery material, obeys the same nature as the earth; it may be that the motion has been communicated to the air, the atmosphere partaking of this motion because of the contact with the earth and the resistance during the constant rotation. Again, an equally astonishing claim, namely, that the highest region of the air obeys the heavenly motion, is said to be supported by those suddenly-appearing stellar objects which are called by the Greeks comets or bearded stars, the origin of which one assigns to just that region, and which, like other constellations, rise and set. It may be said that that part of the air, due to its great remoteness from the earth, has remained immune from the earthly motion. Therefore, the air which lies nearest the earth will appear at rest, as well as those objects floating in it, when they are not driven hither and yon by the wind or by some other

external force, as may happen by chance; for what is the wind in the air other than the waves in the sea? We must admit that the motion of falling and rising objects is, with respect to the universe, a double one, compounded always of rectilinear and circular motions. Since that which, due to its weight, is attracted downwards is essentially earthy, there is no doubt that these parts obey the same law as their whole—namely, the earth; and for the same reason such objects as belong to the fire class are drawn aloft with violence. Earthly fire is fed principally with earthy materials, and it is said that a flame is only burning smoke. The peculiarity of fire, however, consists in expanding that which it has taken hold of; and it achieves this with such violence that it can be hindered by no method or machine from breaking down the barriers and fulfilling its work. But the expanding motion is directed from the center to the periphery. Therefore, when anything composed of earthy parts is ignited, it moves from the center upwards.

Thus, as has been claimed, a simple body has a simple motion and this proves to be preferably a circular motion as long as the simple body remains in its natural position and retains its unity. In this position its motion is merely the circular motion which, being entirely within the body, makes it seem to be at rest. Rectilinear motion, however, attacks bodies which have left or have been forced from their natural positions, or have in some manner become displaced. Nothing militates so against the order and form of the whole world as "being-out-of-its-place." Thus motion in a straight line enters only when things are not in their proper relations and are not completely as they should be, having been separated from their whole and having lost their unity. Moreover, such bodies which are driven upwards or downwards, disregarding the circular motion, do not describe simple uniform and constant motion, for they cannot orient themselves by their lightness or the pressure of their weight; and if at the beginning of their plunge they have a slower motion, they increase their velocity in falling. While on the other hand we see that earthly fire (and we know of no other kind) when driven aloft at once becomes inert, as if it showed by this means the origin of the earthy materials. Circular motion, on the other hand, is always uniform because it has a cause that does not slacken. The other motions, however, diminish during their progress, when the bodies have reached their natural position they cease to be either imponderable or heavy, and, therefore, their motion ceases. If, therefore, the universe possesses circular motion

and its parts possess also rectilinear motion, then we might say that circular motion is compatible with rectilinear motion, just as the animal with disease. If Aristotle divided simple motions into three kinds, away from the center, toward the center, and around the center, that seems to be only an intellectual exercise, just as we distinguish between a line, a point, and a surface, even though one of these cannot exist without the others, and none of them without matter. Moreover, the condition of rest is considered as nobler and more divine than that of change and inconstancy, so the latter would, therefore, be more suited to the earth than to the universe. And I add to this that it seems irrational to ascribe a motion to that which contains and locates and not to that which is contained and is located, namely the earth. Finally, since the planets clearly are now nearer, now farther from the earth, the motion of one and the same body about the center (which is said to be the center of the earth), is also directed away from and toward this center. It is, therefore, necessary to have a more general conception of motion about a center, and it should be sufficient if each single motion has its own center. It is clear, therefore, from all this, that motion of the earth is more probable than rest, especially in relation to the daily rotation, which is most characteristic of the earth.

Chapter IX. Whether the Earth Can Be Assigned Several Motions; and Concerning the Center of the Universe

Since nothing stands in the way of the movability of the earth, I believe we must now investigate whether it also has several motions, so that it can be considered one of the planets. That it is not the center of all the revolutions is proved by the irregular motions of the planets, and their varying distances from the earth, which cannot be explained as concentric circles with the earth at the center. Therefore, since there are several central points, no one will without cause be uncertain whether the center of the universe is the center of gravity of the earth or some other central point. I, at least, am of the opinion that gravity is nothing else than a natural force planted by the divine providence of the Master of the World into its parts, by means of which they, assuming a spherical shape, form a unity and a whole. And it is to be assumed that the impulse is also inherent in the sun and the moon and the other planets, and that by the operation of this force they remain

in the spherical shape in which they appear; while they, nevertheless, complete their revolutions in diverse ways. If then the earth, too, possesses other motions besides that around its center, then they must be of such a character as to become apparent in many ways and in appropriate manners; and among such possible effects we recognize the yearly revolution. If one admits the motionlessness of the sun, and transfers the annual revolution from the sun to the earth, there would result, in the same manner as actually observed, the rising and setting of the constellations and the fixed stars, by means of which they become morning and evening stars; and it will thus become apparent that also the haltings and the backward and forward motion of the planets are not motions of these but of the earth, which lends them the appearance of being actual planetary motions. Finally, one will be convinced that the sun itself occupies the center of the universe. And all this is taught us by the law of sequence in which things follow one upon another and the harmony of the universe; that is, if we only (so to speak) look at the matter with both eyes.

TYCHO BRAHE[1]

On a New Star, Not Previously Seen within the Memory of Any Age since the Beginning of the World

(From "De Nova Stella," Opera Omnia, Tomus I; Edidit I. L. E. Dreyer, 1913; translation by Dr. John H. Walden, 1928.)

Its First Appearance in 1572.—Last year [1572], in the month of November, on the eleventh day of that month, in the evening, after sunset, when, according to my habit, I was contemplating the stars in a clear sky, I noticed that a new and unusual star, surpassing the other stars in brilliancy, was shining almost directly above my head; and since I had, almost from boyhood, known all the stars of the heavens perfectly (there is no great difficulty in attaining that knowledge), it was quite evident to me that there had never before been any star in that place in the sky, even the smallest, to say nothing of a star so conspicuously bright as this. I was so astonished at this sight that I was not ashamed to doubt the trustworthiness of my own eyes. But when I observed that others, too, on having the place pointed out to them, could see that there was really a star there, I had no further doubts. A miracle indeed, either the greatest of all that have occurred in the whole range of nature since the beginning of the world, or one certainly that is to be classed with those attested by the Holy Oracles, the staying of the Sun in its course in answer to the prayers of Joshua, and the darkening of the Sun's face at the time of the Crucifixion. For all philosophers agree, and facts clearly prove it to be the case, that in the ethereal region of the celestial world no change, in the way either of generation or of corruption, takes place; but that the heavens and the celestial bodies in the heavens are without increase or diminution, and that they undergo no alteration, either in number or in size or in light or in any other respect; that they always remain the same, like unto themselves in all respects, no

[1] Tycho Brahe (1546–1601), Danish astronomer, introduced accuracy into the measurement of astronomical positions. His major contributions concern the new star of 1572, the interpretation of comets, the positions of sun, moon, and planets, a catalogue of stars, astronomical instruments, and a plan of the cosmos.

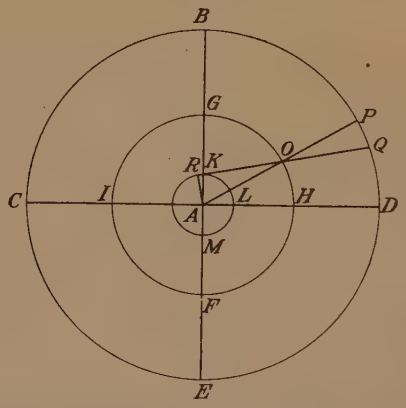
years wearing them away. Furthermore, the observations of all the founders of the science, made some thousands of years ago, testify that all the stars have always retained the same number, position, order, motion, and size as they are found, by careful observation on the part of those who take delight in heavenly phenomena, to preserve even in our own day. Nor do we read that it was ever before noted by any one of the founders that a new star had appeared in the celestial world, except only by Hipparchus, if we are to believe Pliny. For Hipparchus, according to Pliny, (Book II of his Natural History) noticed a star different from all others previously seen, one born in his own age . . .

Its Position with Reference to the Diameter of the World and Its Distance from the Earth, the Center of the Universe.—It is a difficult matter, and one that requires a subtle mind, to try to determine the distances of the stars from us, because they are so incredibly far removed from the earth; nor can it be done in any way more conveniently and with greater certainty than by the measure of the parallax [diurnal], if a star have one. For if a star that is near the horizon is seen in a different place than when it is at its highest point and near the vertex, it is necessarily found in some orbit with respect to which the Earth has a sensible size. How far distant the said orbit is, the size of the parallax compared with the semidiameter of the Earth will make clear. If, however, a [circumpolar] star, that is as near to the horizon [at lower culmination] as to the vertex [at upper culmination], is seen at the same point of the Primum Mobile, there is no doubt that it is situated either in the eighth sphere or not far below it, in an orbit with respect to which the whole Earth is as a point.

In order, therefore, that I might find out in this way whether this star was in the region of the Element or among the celestial orbits, and what its distance was from the Earth itself, I tried to determine whether it had a parallax, and, if so, how great a one; and this I did in the following way: I observed the distance between this star and Schedir of Cassiopeia (for the latter and the new star were both nearly on the meridian), when the star was at its nearest point to the vertex, being only 6 degrees removed from the zenith itself (and for that reason, though it were near the Earth, would produce no parallax in that place, the visual position of the star and the real position then uniting in one point, since the line from the center of the Earth and that from the surface nearly coincide). I made the same observation when the star was farthest from the

Uraniborg, Tycho's Observatory. From *Historia Cælestis*, 1666.

zenith and at its nearest point to the horizon, and in each case I found that the distance from the above-mentioned fixed star was exactly the same, without the variation of a minute: namely 7 degrees and 55 minutes. Then I went through the same process, making numerous observations with other stars. Whence I conclude that this new star has no diversity of aspect, even when it is near the horizon. For otherwise in its least altitude it would have been farther away from the above-mentioned star in the breast of Cassiopeia than when in its greatest altitude. Therefore, we shall find it necessary to place this star, not in the region of the Element, below the Moon, but far above, in an orbit with respect to which the Earth has no sensible size. For if it were in the highest region of the air, below the hollow region of the Lunar sphere, it would, when nearest the horizon, have produced on the circle a sensible variation of altitude from that which it held when near the vertex.



To make the proof clearer, let a circle be drawn representing the meridian, or some other vertical circle of the Primum Mobile, in which the places of all the stars are held to be, and let this circle be *CBDE*, with its center at *A*. Let the diameter *BE* indicate the vertex, and *CD* the horizon. Furthermore, let there be described with the same center a circle *MKL*, which shall indicate the circumference of the Earth. Between these let there be drawn another circle *GHFI*, to represent the lowest circle of the Lunar sphere and the one nearest the Earth, in which we are to imagine this star to be. And let it first be in its greatest altitude, near the point *G*: it is clear that it is entirely without diversity of aspect; for the two lines, one drawn from the center of the Earth, and the

other drawn from the eye placed on the surface of the Earth, unite in one and the same point of the circle of the Primum Mobile *CBDE*, that is, in the point *B*, or near it if the star is not exactly at *G*. For this star is removed 6 degrees from the vertex, when it is for us at its highest point; which distance, however, produces no sensible variation from the vertex itself. But let this star be placed in the same circle *GHFI* at its lowest altitude, which is the point *O*, and, if the eye is placed at *K* on the surface of the Earth, the star will necessarily be seen in another place on the outermost circle from what it will if the eye is at *A*, the center of the Earth. For, if lines are drawn from *K* on the surface, and *A*, the center of the Earth, through *O*, which is the position of the star, to the outermost orbit *BDEC*, the line from *A* through *O* will fall in *P*, while the line from *K* through the same point *O* will fall in *Q*. *PQ*, therefore, is the arc of the Primum Mobile showing the diversity of aspect of the star.

I will try to determine, therefore, the length of the arc *PQ*, so that we may learn how great is the diversity of aspect which this star has when it is at its nearest point to the horizon, if it is placed in the circle *IGHF*, immediately below the orbit of the Moon, at the point *O*. That this may be done more conveniently, let the line *QOK* be produced until another line drawn from the center *A* meets it perpendicularly, and let the point of meeting be *R*. Since the angle *BKO* is known by observation—for it is the complement of the least altitude of the star itself, namely 62 degrees, 5 minutes —its vertical angle *RKA* will be known, being its equal. Furthermore, the angle *KRA* is by hypothesis a right angle; and the side *KA* is known by some measurement or other, for it is the semidiameter of the Earth itself. *AR* will be found by Proposition 29 of Regiomontanus concerning plane triangles. If, therefore, we give to the semidiameter of the Earth, *KA*, as being the whole sine, since it is the side opposite the right angle *R*, the length of 100,000 units, the side *AR* proves to be 88,363 units. Now at last I form my concept of the triangle *ROA*, two sides of which, *RA* and *AO*, are known. For *AO* is the distance from the center of the Earth to the lowest surface of the orbit of the Moon, which distance, with Copernicus, I have set at 5,200,000 of the same units in which the semidiameter of the Earth, *AK*, was reckoned at 100,000 (for I find it best to make use of larger numbers in this computation, that the calculation may be carried on more conveniently and the result be given more exactly); and since in the afore-mentioned triangle

the angle *ORA* is by hypothesis a right angle, the angle *ROA* will be found by the 27th Proposition of Regiomontanus on plane triangles. For by multiplying the side *AR* into the whole sine, we get 8,836,300,000, which number, being divided by the side *AO*, gives 1699 units, the sine, namely, of the angle *ROA*, whose arc is 0 degrees, 58½ minutes; and this number determines the size of the required angle. To this angle, *ROA*, the angle *POQ* is equal, since it is its vertical angle, as is manifest from the principles of geometry. Therefore, the arc *PQ*, which is the measure of this angle (for, owing to the immense distance between the Lunar sphere and the Primum Mobile, the arc *PQ* does not differ sensibly from the arc of the circle intercepted by the same lines at the distance *OP*) and indicates the parallax of the star, will be 58½ minutes, which was what we had to find. So great, therefore, would have been the diversity of aspect of this star in the position *O*, as between that place which it held near the vertex and that in which it was seen when nearest the horizon. But after making many careful observations, as I said above, with a most delicate and accurate instrument, I found that this was not the case. Whence I conclude that this star which has recently become visible is not in the circle *IGHF*, in the uppermost region, that is, of the air, immediately below the orbit of the Moon, nor in any place yet nearer the Earth—for in the latter case the arc *PQ* would have produced a greater length, and the diversity of aspect would be greater—but that it is situated far above the Lunar sphere, in the heaven itself, and in fact in some orbit so far removed from the Earth that the line *KA*, the semidiameter of the Earth, has no sensible size in respect to it, but that the whole Earth, when compared to it, is observed to be no more than a point; and this has been found by the founders of the science to be in the eighth sphere or not far from it, in the higher orbits of the three superior planets. Whence this star will be placed in the heavens themselves, either in the eighth orbit with the other fixed stars or in the spheres which are immediately beneath it. That it is not in the orbit of Saturn, however, or in that of Jupiter, or in that of Mars, or in that of any one of the other planets, is clear from this fact: after the lapse of six months it had not advanced by its own motion a single minute from that place in which I first saw it; and this it must have done if it were in some planetary orbit. For, unlike the Primum Mobile, it would be moved by the peculiar motion of the orbit itself, unless it were at rest at one or the other pole of the orbits of the Secundum

Mobile; from which, however, as I have shown above, it is removed 28 degrees. For the entire orbits, revolving on their own poles, carry along their own stars, or (as I see Pliny and some others hold) are carried along by them; unless, indeed, one would deny the belief accepted by philosophers and mathematicians, and assert (what is absurd) that the stars alone revolve, while the orbits are fixed. Therefore, if this star were placed in some one of the orbits of the seven wandering stars, it would necessarily be carried around with the orbit itself to which it were affixed, in the opposite direction to the daily revolution. And, furthermore, this motion, even in the case of the orbit which moves the slowest, that of Saturn, would, after such a length of time, be noticed, though one were to make his observation without any instrument at all.

Therefore, this new star is neither in the region of the Element, below the Moon, nor among the orbits of the seven wandering stars, but it is in the eighth sphere, among the other fixed stars, which was what we had to prove. Hence it follows that it is not some peculiar kind of comet or some other kind of fiery meteor become visible. For none of these are generated in the heavens themselves, but they are below the Moon, in the upper region of the air, as all philosophers testify; unless one would believe with Albategnius that comets are produced, not in the air, but in the heavens. For he believes that he has observed a comet above the Moon, in the sphere of Venus. That this can be the case, is not yet clear to me. But, please God, sometime, if a comet shows itself in our age, I will investigate the truth of the matter. Even should we assume that it can happen (which I, in company with other philosophers, can hardly admit), still it does not follow that this star is a kind of comet; first, by reason of its very form, which is the same as the form of the real stars and different from the form of all the comets hitherto seen, and then because, in such a length of time, it advances neither latitudinally nor longitudinally by any motion of its own, as comets have been observed to do. For, although these sometimes seem to remain in one place several days, still, when the observation is made carefully by exact instruments, they are seen not to keep the same position for so very long or so very exactly. I conclude, therefore, that this star is not some kind of comet or a fiery meteor, whether these be generated beneath the Moon or above the Moon, but that it is a star shining in the firmament itself—one that has never previously been seen before our time, in any age since the beginning of the world.

Drawing of the constellation of the Little Bear; Polaris, the north pole star, is at the end of the bear's tail.
From Bayer's *Uranometria*, 1603.

BAYER[1]

The Constellations

(From "Uranometria," 1603.)

PARS PRIMA.

Complectens, nomina omnium conſtellationum, quæ ab Ecliptica ad eius polum Boreum vergunt, vnà cum ordine, numero antiquo, nouiterq; adaucto, longitudinibus, latitudinibus, atquè magnitudinibus Stellarum.

TABVLA PRIMA.

VRSA MINOR.

SEptentrio, Arctos minor, Cynoſura, Phænice, Ἀμαξα, Plauſtrum ſeu Ploſtrum minus, Eruccabah, Ezra.

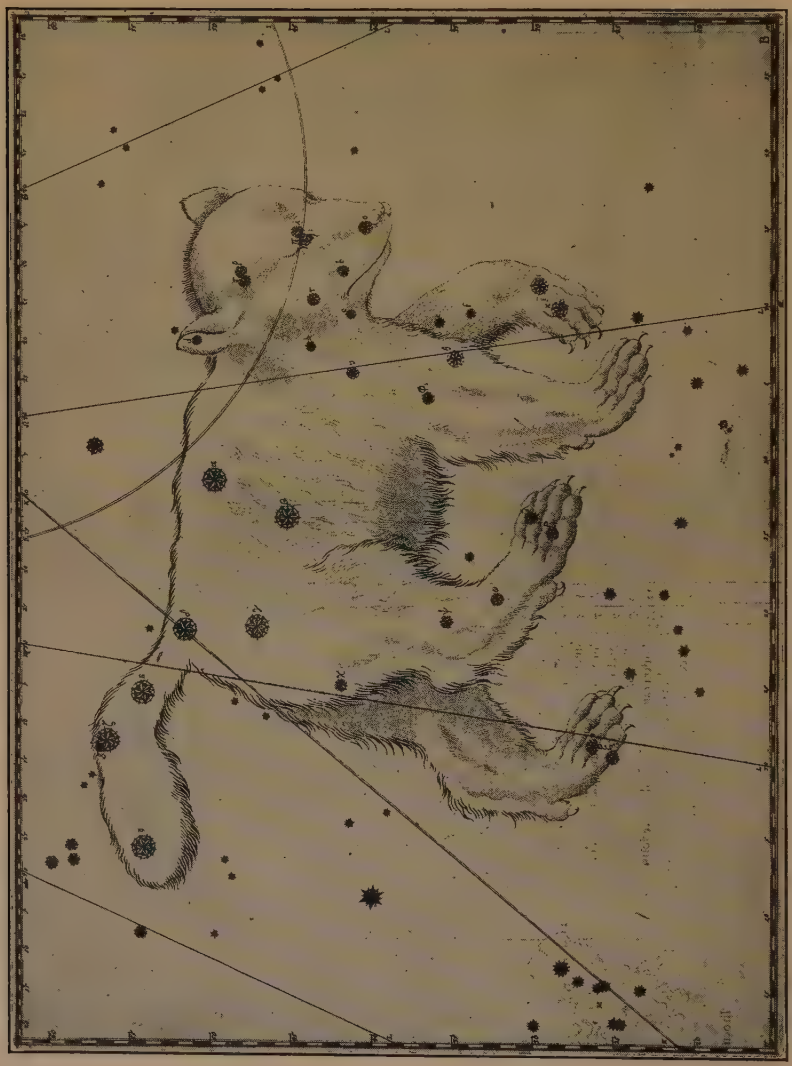
DIARTHROSIS.

1	α	CAuda extima, ſeu ſtella polaris, nauigatoria, Stella maris, la tramontana, Alruccabah, ſeu Ruccabah Iſmaëlitis.	2 Secundæ.	differentiæ.
6	ϐ	Earum quæ in ſeq: latere auſtralior. Kochab.		
7	γ	Eiusdem lateris borealior.	1 Tertiæ.	
2	δ	Sequens ab Alruccabah, χορευτὴς πρῶτη.		
3	ε	In eductione caudæ, χορευτὴς δευτέρα.	3 Quartæ.	
4	ζ	In latere quadranguli præcedente, auſtralis.		
5	η	Eiusdem lateris borealior.	1 Quintæ.	
8	θ	In latere quadranguli p̄cedente, auſtralior.	1 Sextæ.	

Omnes de natura ♂ & ♀.

Partial list of stars in the constellation of the Little Bear.

[1] John Bayer (1572–1625), Bavarian attorney. Concerning his work, B. A. Gould wrote in the Uranometria Argentina (1879) as follows: "As regards our knowledge of the true aspect and magnitudes of the stars, Bayer occupies the same place in history as those other great men who in their several branches of science broke loose to some extent, although only too partially, from the fetters of tradition and dogma, appealing from the ἄυτοσέφη of some ancient authority to Nature herself. The magnitudes assigned to the principal stars had become traditional and conventional to an extent that now seems hardly



Drawing of the constellation of the Big Bear. From Bayer's *Uranometria*, 1603.

TABVLA SECVNDA.

VRSA MAIOR.

Cynoſuris, Plauſtriluca, Licaonia, Parrhaſis, Mænalis, Erymanthis, Nonacrina, Septentrio, Arctos maior, Maxima Hyginio, Magna Ouidio, Αμαξα μεγὰ, Plauſtrum ſeu Ploſtrum maius, Germanis Hôrwagen/ Ελικῶπις Homero, Helice, Calliſto, Megiſto, Elix, Arcturus, Dubhelacbar, in tabulis Elkeid.

DIARTHROSIS.

16	α	Quæ in Humero, Dubhe, πρώτη τῆ πλινθίω.	Septem Triones.	Secundæ.
17	β	Quę in Ilibus, δευτέρα τῆ πλινθίω.		
19	γ	In Siniſtro crure poſteriore. τρίτη τῆ πλινθίω.		
18	δ	In eductione caudæ. τετάρτη τῆ πλινθίω.		
25	ε	Prima poſt eductionem, Λαγὼν, Υπόζωμα, Risalioth, Mirach, rectiùs Aliath, Micar, vel Mizar.		
26	ζ	Media earum.		
27	η	Extrema cauda, Benenaim, Benenatz, correctiùs Benetnaſch, Elkeid.		
11	θ	In genu ſiniſtro anteriore.	3 Tertiæ.	differentiæ.
12	ι	Duarum in extremo pedis dextri, borealis.		
13	κ	Altera auſtralis.		
20	λ	Duarum p̃cedens in pede ſiniſtro poſteriore.	14 Quartæ.	
21	μ	Sequens.		
23	ν	Duarũ quę in pede dextro poſteriore, borealis.		
24	ξ	Quæ magis ad auſtrum.		
1	ο	In naſo, barbaris muſcidâ.		
3	π	In oculo auſtralior.		
4	ρ	In fronte præcedens.		
5	σ	Sequens.		
8	τ	Infima & ſequens in paruo colli triangulo.		
9	υ	In pectore borealior.		
10	φ	Auſtralior.		
22	χ	In ſiniſtrâ cauitate pedis ſiniſtri poſterioris.		
28	ψ	In eiusdem genu ſequens.		
29	ω	Antecedens.		

List of stars in the constellation of the Big Bear (Big Dipper).

Drawing of the constellation of Orion. From Bayer's *Uranometria*, 1603.

TABVLA TRIGESIMA QVINTA.

ORION.

ΩΡΙΩΝ, ΩΑΡΙΩΝ, Arion, Hyriades, Audax,

Furiosus, Sublimatus, Gigas, Bellator fortissimus Latinis Iugula, Arab. Elgeuze, Sugia, Asugia, Elgebar, Algebar, Algebra, Kesil, Geuze.

DIARTHROSIS.

2	α	IN humero sinistro lucida rubescens, Betelgeuze. Aratus γλήνεα vocat. ♂♃.	2	Primi.
35	β	In extremo pede dextro, Rigel, Elgebar, Kesil; κοινὸς τοῦ ὠρίωνος καὶ τοῦ ποταμοῦ ἀστήρ. ♃.		
3	γ	In humero læuo duarum Borealior. Bellatrix.	4	Secundi.
26	δ	In baltheo fulgentium trium præcedens.		
27	ε	Media.		
28	ζ	Sequens. Has tres vulgus S. Iacobi bæculum indigetat.		
29	η	Sub baltheo trium inferior.	4	Tertii.
31	θ	In ense tertia.		
32	ι	Quarta.		
38	κ	Ad genu sinistrum.		
1	λ	In capite trium superior. ♂♃.	16	Quarti.
5	μ	Ἐν τῷ προπηχίῳ læuo.		
8	ν	In manu Borealior.		
7	ξ	Australior.		
18	ο	Septentrionales duæ, ἐν τῇ δορᾷ.		
25	π	Sex aliæ descendentes in Austrum.		
39	ρ	Sub axillâ dextrâ, ἐν τῇ δορᾷ.		
40	σ	In ense, prima.		
36	τ	In dextrâ surâ trium præcedens.		Fulgoris.
33	υ	Sequens.		
42	φ	In capite trium media & Australior. ♂♃.	11	Quinti.
12	χ	Duæ in clauâ.		
13	ψ	In dorso præcedens secunda.		
16	ω	Quinta vel vltima.		
4	A	In humero dextro duarum Australior.		
43	b	In ensis capulo.		
30	c	In ense, secunda.		
34	d	In femine læuo.		
37	e	Trium dextræ suræ, media.		

List of stars in the constellation of Orion.

Drawing of the Zodiacal Constellations. From Bayer's *Uranometria*, 1603.

Drawing of the Zodiacal Constellations. From Bayer's *Uranometria*, 1603.

credible; even Tycho was wont to assign to the stars, which he himself observed, magnitudes taken, not from the sky, but from the Almagest.

"The simple and important advance, of so delineating the constellations as to permit the maps to be compared directly with the face of the sky, rather than with a fictitious outer surface of the imaginary celestial sphere, would seem to be such as to commend itself to the favor of all astronomers; yet such was not the case. It necessarily implied a reversion of some of the figures, thus interchanging right and left; and this gave rise, not only to the vehement opposition of Bartsch, Shickard, and others, but even of those great observers, Hevel and Flamsteed. The last-named astronomer attributed the inversion to a misinterpretation of Ptolemy's phraseology, and in a philological dissertation disproved the supposed error. Even now it is customary for popular treatises on astronomy to quote the ill-advised remark of Montucla, that Bayer probably erred from overlooking the inversion produced by printing from an engraved plate; as likewise, the yet more flippant remark of Delambre, that no astronomer ever yet gained immortality so cheaply as did Bayer by merely assigning Greek and Roman letters to the stars, for names.

"The *Uranometria* of Bayer was a scientific work, the result of thought, study, and laborious observation. It first placed on record the approximate positions and the magnitudes of some 500 stars in addition to the 777 which formed the renowned catalogue published by Tycho Brahe, only one year previous. And although adopting for Tycho's stars, without question, the magnitudes as well as the positions assigned by him, it gave for the new stars, what the catalogue of that illustrious astronomer did not always give, magnitudes derived directly from observation; so that these may be accepted as a truthful record, for future ages, of their aspect as that important epoch . . .

"The system which he adopted for his notation was not, as has been supposed by many, that of assigning the letters in the constellation in the alphabetical order corresponding to the order of brightness of the several stars. On the contrary, as Argelander has conclusively shown, Bayer made no attempt at other discrimination of the relative brightness of the stars, than according to the six orders of magnitude, which had been transmitted from antiquity. For the stars of each order, the sequence of the letters in no manner represented that of their brightness, but depended upon the position of the stars in the figure; beginning usually at the head, and following its course until all the stars of that order of magnitude were exhausted. The non-recognition of this rule, and of the fact that the magnitudes of Ptolemy's and Tycho's stars were taken directly from the *Almagest* and *Progymnasmata*, has led to numerous false theories, and to unwarranted depreciation of the results of this remarkable and truly important work."

KEPLER[1]

Reconcilings of Texts of Sacred Scripture That Seem to Oppose the Doctrine of the Earth's Mobility

(From "Introduction upon Mars," Mathematical Collections and Translations, Salusbury, 1661.)

It must be confessed, that there are very many who are devoted to Holiness, that dissent from the Judgment of *Copernicus*, fearing to give the Lye to the Holy Ghost speaking in the Scriptures, if they should say, that the Earth moveth, and the Sun stands still. But let us consider, that since we judge of very many, and those the most principal things by the Sense of Seeing, it is impossible that we should alienate our Speech from this Sense of our Eyes. Therefore, many things daily occur, of which we speak according to the Sense of Sight, when as we certainly know that the things themselves are otherwise. An example whereof we have in that Verse of *Virgil;*

Provehimur portu, Terraeque urbesque recedunt . . .

And I do also beseech my Reader, not forgetting the Divine Goodnesse conferred on Mankind; the consideration of which the Psalmist doth chiefly urge, that when he returneth from the Temple, and enters into the School of *Astronomy*, he would with me praise and admire the Wisdome and Greatnesse of the Creator, which I discover to him by a more narrow explication of the World's Form, the Disquisition of Causes, and Detection of the

[1] John Kepler (1571–1630), German astronomer, "analyzed Tycho's observations to find the true law of motion of Mars. After incredible labour, through innumerable wrong guesses, and six years of almost incessant calculation, he at length emerged in his two 'laws'—discoveries which swept away all epicycles, deferents, equants, and other remnants of the Greek system, and ushered in the dawn of modern astronomy" (Lodge). His mystic temperament led to the happy inspiration of trying to get a relation connecting the size of orbits with times of revolution and finally to the discovery of the "third law," which is given in "Harmony of the World." His contributions include commentaries on the motion of Mars, an epitome of the Copernican astronomy, a treatise on comets, on a new star, and on optics, and the Rudolphine Tables.

Errours of Sight: And so he will not onely extoll the Bounty of God in the preservation of Living Creatures of all kindes, and establishment of the Earth; but even in its Motion also, which is so strange, so admirable, he will acknowledge the Wisdome of the Creator. But he who is so stupid as not to comprehend the Science of *Astronomy*, or so weak and scrupulous as to think it an offence of Piety to adhere to *Copernicus*, him I advise, that leaving the Study of *Astronomy*, and censuring the opinions of Philosophers at pleasure, he betake himself to his own concerns, and that desisting from further pursuit of these intricate Studies, he keep at home and manure his own Ground; and with those Eyes wherewith alone he seeth, being elevated towards this to be admired Heaven, let him pour forth his whole heart in thanks and praises to God the Creator; and assure himself that he shall therein perform as much Worship to God, as the *Astronomer*, on whom God hath bestowed this Gift, that though he seeth more clearly with the Eye of his Understanding; yet whatever he hath attained to, he is both able and willing to extoll his God above it.

And thus much concerning the Authority of Sacred Scripture. Now as touching the opinions of the Saints about these Natural Points. I answer in one word, That in Theology the weight of Authority, but in Philosophy the weight of Reason is to be considered. Therefore Sacred was *Lactantius*, who denyed the Earth's rotundity; Sacred was *Augustine*, who granted the Earth to be round, but denyed the *Antipodes;* Sacred is the Liturgy (Officium) of our Moderns, who admit the smallnesse of the Earth, but deny its Motion: But to me more sacred than all these is Truth, who with respect to the Doctors of the Church, do demonstrate from Philosophy that the Earth is both round, circumhabited by *Antipodes*, of a most contemptible smallnesse and in a word, that it is ranked amongst the Planets.

The Discovery of the Laws of Planetary Motion

Chief Points of Astronomical Learning, Necessary for the Contemplation of the Celestial Harmonies

(From "Harmonice Mundi," Opera Omnia, Volumen Quintum; Edidit Dr. Ch. Frisch, 1864; translation by Dr. John H. Walden, 1928.)

In the beginning let my readers understand this: that the old astronomical hypotheses of Ptolemy, as they are set forth in the

Theoriae of Purbach and the writings of the other epitomizers, are to be kept far from the present enquiry and banished wholly from the mind; for they fail to give a true account either of the arrangement of the heavenly bodies or of the laws governing their motions.

In their place I cannot do otherwise than substitute simply Copernicus's theory of the universe, and (were it possible) convince all men of its truth; but, since among the mass of students the idea is still unfamiliar, and the theory that the Earth is one of the planets and moves among the stars about the Sun, which is stationary, sounds to the most of them quite absurd, let those who are offended by the strangeness of this doctrine know that these harmonic speculations hold a place even among the hypotheses of Tycho Brahe. While that author agrees with Copernicus in regard to everything else which concerns the arrangement of the heavenly bodies and the laws governing their motions, the annual motion of the Earth alone, as held by Copernicus, he transfers to the whole system of the planetary orbits and to the Sun, which, according to both authors, is the center of the system. For from this transference, motion results just the same, so that, if not in that utterly vast and immense space of the sphere of the fixed stars, at least in the system of the planetary world, the Earth holds at any one time the same place according to Brahe as is given to it by Copernicus. Furthermore, just as he who draws a circle on paper moves the writing foot of the compass around, while he who fastens the paper or a board to a revolving wheel keeps the foot of the compass or the style stationary and draws the same circle on the moving board, so also in the present case; for Copernicus, the Earth measures out its orbit, between the outer circle of Mars and the inner circle of Venus, by the real motion of its own body, while for Tycho Brahe the whole planetary system (in which among the other orbits are also those of Mars and Venus) turns around like the board on the wheel and brings to the stationary Earth, as to the style of the turner, the space between the orbits of Mars and Venus; and from this motion of the system it results that the Earth, itself remaining stationary, marks on space the same course around the Sun, between Mars and Venus, which, according to Copernicus, it marks by the real motion of its own body with the system at rest. Since, then, the harmonic speculation considers the eccentric motions of the planets, as seen from the Sun, one can easily understand that, if an observer were on the Sun, however great the Sun's motion,

the Earth, although it were at rest (to grant this for the moment to Brahe), would, nevertheless, seem to him to run its annual course in the space between the planets, and also in a time between the planet's times. Although, therefore, a man may be weak in faith and so unable to conceive of the motion of the Earth among the stars, he may still find it possible to take pleasure in the exalted contemplation of this most divine mechanism; he needs but to apply whatever he hears about the daily motions of the Earth in its eccentric to the appearance of those motions on the Sun, as even Tycho Brahe presents it with the Earth at rest.

The true followers of the Samian philosophy, however, have no just cause for envying such men this participation in a most delightful speculation, for if they accept also the immovability of the Sun and the motion of the Earth, their pleasure will be more exquisite in many ways, since it will be derived from the very consummated perfection of contemplation.

In the first place, therefore, let my readers understand that at the present day among all astronomers it is held to be a well-established fact that all the planets except the Moon, which alone has the Earth as its center, revolve around the Sun; the Moon's orbit or course, be it said, is not large enough to enable it to be drawn on this chart in proper relation to the other orbits. To the other five planets, therefore, is added the Earth as sixth, which, either by its own motion, with the Sun stationary, or, itself being at rest while the whole planetary system is in revolution, describes, it too, its orbit, the sixth, about the Sun.

Secondly, the following fact is also established: that all the planets revolve in eccentric orbits; that is, they alter their distances from the Sun, so that in one part of the orbit they are very remote from the Sun, while in the opposite part they come very near the Sun. In the appended scheme there have been made for each planet three circles, no one of which indicates the real eccentric path of the planet; the middle one, however, as, for instance, in the case of Mars, *BE*, has a diameter equal to the longer diameter of the eccentric orbit; the orbit itself, as *AD*, touches *AF*, the highest of the three, in the one quarter, *A*, and *CD* the lowest, in the other quarter, *D*.

The orbit *GH*, represented by points and drawn through the center of the Sun, indicates the path of the Sun according to Tycho Brahe. If the Sun travels this path, every point of the planetary system here depicted advances in a like path, each in its own;

and if one point of it, that is the center of the Sun, stands in one part of its orbit, as here in the lowest part, all parts of the system will stand, each in the lowest part of its own orbit. Owing to the narrowness of the space, the three circles of Venus have run into one, contrary to my intention.

Thirdly, let the reader recall from my "Mysterium Cosmographicum," which I published twenty-two years ago, that the number of the planets, or orbits about the Sun, was derived by the most wise Creator from the five solid figures, about which Euclid so many centuries ago wrote the book which, since it is made up of a series of propositions, is called "Elementa." That there cannot be more regular bodies, that regular plane figures, that is, cannot unite into a solid in more than five ways, was made clear in the second book of the present work.

Fourthly, as regards the relations of the planetary orbits, the relation between two neighboring orbits is always such that, as will easily be seen, each one of the orbits approximates one of the terms of the ratio which exists between the orbits of one of the five solid bodies;[1] the ratio, that is, of the orbit circumscribed about the figure to the orbit inscribed. For when, following the observations of

[1] Or "they are approximately proportional to the orbits of one of the five solid figures." (*Note by translator.*)

Brahe, I had completed the demonstration of the distances, I discovered this fact: if the angles of the cube are applied to the innermost circle of Saturn, the centers of the planes nearly touch the middle circle of Jupiter, and if the angles of the tetrahedron rest on the innermost circle of Jupiter, the centers of the planes of the tetrahedron nearly touch the outermost circle of Mars; also, if the angles of the octahedron rise from any one of the circles of Venus (for all three are reduced to a very narrow space), the centers of the planes of the octahedron enter and descend below the outermost circle of Mercury; finally, coming to the ratios which exist between the orbits of the dodecahedron and the orbits of the icosahedron, which ratios are equal to each other, we find that the nearest of all to these are the ratios or distances between the circles of Mars and the Earth and between those of the Earth and Venus, and these ratios also, if we reckon from the innermost circle of Mars to the middle circle of the Earth and from the middle circle of the Earth to the middle circle of Venus, are similarly equal to each other; for the middle distance of the Earth is the mean proportional between the smallest distance of Mars and the middle distance of Venus; but these two ratios between the circles of the planets are still larger than are the ratios of those two sets of orbits in the figures, so that the centers of the planes of the dodecahedron do not touch the outermost circle of the Earth, nor do the centers of the planes of the icosahedron touch the outermost circle of Venus; and this hiatus is not filled up by the semidiameter of the orbit of the Moon, added to the greatest distance of the Earth and taken away from the smallest distance. But there is a certain other relation connected with a figure that I notice: if an enlarged dodecahedron to which I have given the name *echinus* [hedgehog] as being formed of twelve five-cornered stars and thereby being very near to the five regular bodies, if, I say, this dodecahedron should place its twelve points on the innermost circle of Mars, then the sides of the pentagons, which are, respectively, the bases of the different radii or points, touch the middle circle of Venus.

Briefly: the cube and the octahedron enter somewhat their conjugate planetary orbits, the dodecahedron and the icosahedron do not quite reach their conjugate orbits, the tetrahedron just touches both orbits; in the first case there is a deficiency, in the second case an excess, in the last case an equality, in the distances of the planets.

From these considerations it is apparent that the exact relations of the planetary distances were not derived from the regular figures alone; for the Creator, the very fountain head of geometry, who, as Plato says, practises geometry eternally, does not deviate from his archetype. And indeed this fact might be gathered from the consideration that all the planets change their distances through definite periods of time; so that each one has two notable distances from the Sun, the maximum and the minimum; and there may be made between every two planets a fourfold comparison of their distances from the Sun, comparisons of their maximum and of their minimum distances, and comparisons of their mutually opposed distances, those that are farthest apart and those that are nearest together; thus, of all the combinations of two neighboring planets, the comparisons are twenty in number, while on the other hand the solid figures are but five. It is reasonable to believe, however, that the Creator, if he paid attention to the relation of the orbits in their general aspect, paid attention also to the relation of the varying distances of the individual orbits in detail, and that these acts of attention were the same in both cases and were connected with each other. When we duly consider this fact, we shall certainly arrive at the conclusion that for establishing the diameters and the eccentricities of the orbits there are required several principles in combination, besides the principle of the five regular bodies.

Fifthly, to come to the motions, among which are established the harmonies, I again impress upon the reader the fact that it has been shown by me in my Commentaries on Mars, from the exceedingly accurate observations of Brahe, that equal diurnal arcs on one and the same eccentric are not traversed with equal velocities, but that these different times *in equal parts of the eccentric are to each other as the distances from the Sun*, the source of the motion; and, on the other hand, that, the times being supposed equal, as, for instance, one natural day in each case, *the true diurnal arcs corresponding to them in a single eccentric orbit are inversely proportional to the two distances from the Sun.* It has likewise been shown by me that *the orbit of a planet is elliptical, and the Sun, the source of motion, is in one of the foci of this ellipse, and so it results that the planet, when it has completed a quarter of the entire circuit, beginning at the aphelion, is at a distance from the Sun exactly half way between the maximum distance in aphelion and the minimum distance in perihelion.* From these two axioms it results that *the mean diurnal*

motion of the planet in its eccentric is the same as the real diurnal arc of that eccentric at the moments at which the planet is at the end of the quarter eccentric reckoned from the aphelion, although that true quadrant as yet appears smaller than the exact quadrant. It follows, further, that *any two perfectly exact diurnal arcs of the eccentric, at exactly the same distance, the one from the aphelion, the other from the perihelion, are together equal to two median diurnal* arcs; and consequently, that, *since circumferences are to each other as diameters, one mean diurnal arc is to the sum of all the mean arcs, which are equal to each other, as many as there are in the whole circumference, as one mean diurnal arc is to the sum of all the real eccentric arcs, the same in number but unequal to each other.* And these truths concerning the real diurnal arcs of the eccentric and the real motions must be known beforehand, that now from these we may understand the apparent motions as they are when observed from the Sun.

Sixthly, as regards the apparent arcs as seen from the Sun, it is known even from the ancient astronomy that of real motions, even when they are equal to each other, that which is farther from the center of the universe (as one that is in aphelion) appears to the eye looking at it from that center to be less, and that which is nearer (as one that is in perihelion) seems to be greater. Since, therefore, in addition, the real diurnal arcs which are in proximity are greater still on account of the greater velocity, and the real arcs in the remote aphelion are smaller still on account of the retardation, it results, as I have shown in my Commentaries on Mars, that *the apparent diurnal arcs of one eccentric are almost exactly inversely proportional to the square of their distances from the Sun.*[1] As, for instance, if a planet in one of its days when it is in aphelion is distant from the Sun 10 units, in any measure whatsoever, and in its opposite day, when it is in perihelion, is distant 9 units of exactly the same kind, it is certain that, as seen from the Sun, its apparent progress in aphelion will be to its apparent progress in perihelion as 81 is to 100.

Now this is true with these reservations: first, that the arcs of the eccentric be not large, that they may not have different distances varying greatly, that is, that they may not cause a sensible variation in the distances of their ends from the apsides; secondly, that the eccentricity be not very great, for the greater the eccen-

[1] Or "the ratio of the apparent diurnal arcs of one eccentric is almost exactly twice the inverse of the ratio of their distances from the Sun." $\frac{1}{2} \times \frac{1}{2}$ = twice $\frac{1}{2}$. (*Note by translator.*)

tricity, that is the greater the arc, the greater is the increase of the angle of that appearance in comparison with its own advance toward the Sun, according to Theorem 8 of the "Optics" of Euclid. But there is another reason why I give this warning. The arcs of the eccentric about the middle of the anomalies are observed obliquely from the center of the Sun, and this obliquity diminishes the size of their appearance, while, on the other hand, the arcs around the apsides are presented to the sight, which is supposed to be on the Sun, from directly in front. When, therefore, the eccentricity is very great, the relation of the motions is sensibly disarranged if we apply the mean diurnal motion without diminution to the mean distance, as if it appeared from the mean distance as large as it is; and this will appear below in the case of Mercury. All this matter is treated at greater length in "Epitome Astronomiae Copernicae," Book V, but it had to be given here because it concerns the very terms themselves of the celestial harmonies, when considered apart each by itself.

Seventhly, in case anyone chances to think of those diurnal motions that are apparent, not to the assumed observer on the Sun, but to the observer on the Earth, with regard to which motions Book VI of "Epitome Astronomiae Copernicae" deals, let him know that these do not come under consideration at all in the present enquiry; clearly they should not, since the Earth is not the source of their motion, nor can they, since these motions, being referred to a false appearance, change not only into absolute rest or apparent motionlessness, but even into retrograde motion; whereby all the infinity of relations is attributed to all the planets at one and the same time and equally. That we may determine, therefore, what the inherent relations are that are established by the diurnal motions of the true individual eccentric orbits (although as yet even they are apparent, being supposed to be seen from the Sun, the source of motion), we must first separate from these inherent motions this appearance of extrinsic annual motion common to all five planets, whether that motion is due, as Copernicus holds, to the motion of the Earth itself, or, as Tycho Brahe holds, to the annual motion of the whole system, and these motions peculiar to each planet must be presented to our view freed from what is extraneous.

Eighthly, thus far we have dealt with the various times of arcs of one and the same planet. Now we must deal also with the motions of the planets taken two at a time and compare these

motions with each other. And here note the definition of the terms that we shall find it necessary to use. By the *proximate apsides* of two planets we shall mean the perihelion of the higher and the aphelion of the lower, notwithstanding the fact that they turn not toward the same quarter of the heavens, but toward different and possibly opposite quarters. *Extreme motions*, understand to be the slowest and the fastest of the entire planetary circuit; *convergent extreme* or *converse*, those that are in the nearest apsides of two orbits, that is, in the perihelion of the superior, and the aphelion of the inferior; *divergent* or *diverse*, those that are in opposite apsides, that is in the aphelion of the superior, and the perihelion of the inferior. Again, therefore, a part of my "Mysterium Cosmographicum," suspended twenty-two years ago, because I did not then see my way clear, must be completed and introduced here. For, after I had by unceasing toil through a long period of time, using the observations of Brahe, discovered the true distances of the orbits, at last, at last, the true relation of the periodic times to the orbits and, if you ask for the exact time,

> . . . though late, yet looked upon me idle
> And after long time came;

conceived on the 8th of March of this year, 1618, but unsuccessfully brought to the test and for that reason rejected as false, but, finally returning on the 15th of May, by a new onset it overcame by storm the shadows of my mind, with such fullness of agreement between my seventeen-years' labor on the observations of Brahe and this present study of mine that I at first believed that I was dreaming and was assuming as an accepted principle what was still a subject of enquiry. But the principle is unquestionably true and quite exact: *the periodic times of any two planets are to each other exactly as the cubes of the square roots of their median distances;*[1] this fact should be observed, however, that the arithmetic mean between the two diameters of the elliptical orbit is a little less than the longer diameter. And so, if one takes from the period, say, of the Earth, which is one year, and from Saturn's period of thirty years, the third part of the ratio, that is the cubic roots, and doubles this ratio by squaring the roots, one has in the resulting numbers the exact ratio of the median distances from the Sun of

[1] Or "the ratio that exists between the periodic times of any two planets is exactly one and one-half times the ratio of their median distances, that is of the orbits themselves." $\frac{3}{2}$ = the cube of the square root. (*Note by translator.*)

the Earth and Saturn.[1] For the cubic root of 1 is 1 and the square of that is 1; and the cubic root of 30 is greater than 3, and the square of that, therefore, is greater than 9. And Saturn, when at its mean distance from the Sun, is a little higher than nine times the mean distance of the Earth from the Sun.

Ninthly, if now you wish to measure as by the same ten-foot rule the exact journeys made by each planet daily through the sky, you will have to combine two ratios, one of the real (not apparent) daily arcs of the eccentric, the other of the mean distance of each planet from the Sun, because this is likewise the ratio of the amplitudes of the orbits; that is, the real daily arc of each planet must be multiplied into the semidiameter of its own orbit. This done, there will result numbers suitable for use in ascertaining whether those journeys have harmonic relations.

Tenthly, that you may know how great the apparent length of any such daily journey is when the eye is supposed to be on the Sun—although this may be obtained directly from astronomical observation, still it will also result if you add to the ratio of the journeys the inverse ratio of the mean, not real, distances of any point of the eccentrics, the journey of the superior eccentric being multiplied into the distance from the Sun of the inferior, and, on the other hand, the journey of the inferior being multiplied into the distance from the Sun of the superior.

Eleventhly, furthermore, given the apparent motions, the aphelion of one and the perihelion of the other, or conversely, or alternately, there are elicited ratios of the distances, of the aphelion of one to the perihelion of the other; in which case, however, the mean motions must be known beforehand, that is the inverse ratio of the periodic times, from which is deduced the proportion relating to the orbits found in paragraph VIII. Then, *taking the mean proportional between either apparent motion and its own mean, the result is that, as this mean proportional is to the semidiameter of its orbit* (which is already given), *so is the mean motion to the distance or interval sought.* Let the periodic times of two planets be 27 and 8; then their mean diurnal motions are to each other as 8 is to 27. Therefore, the semidiameters of the orbits will be as 9 is to 4. For the cubic root of 27 is 3, and that of 8 is 2, and the squares of these roots, 3 and 2, are 9 and 4. Now let the apparent

[1] For in my Commentaries on Mars, Chap. XLIII, fol. 232 (III, 353) I showed that this arithmetic mean is either the diameter itself of the circle which is equal in length to the elliptical orbit or very little less than the diameter.

motions be, the aphelion of one 2, and the perihelion of the other 33⅓. The mean proportionals between the mean motions, 8 and 27, and these apparent motions will be 4 and 30. If, therefore, the mean 4 gives the mean distance of the planet 9, then the mean motion 8 gives the aphelion distance 18, corresponding to the apparent motion 2; and if the other mean 30 gives the mean distance of the other planet 4, then the mean motion of that planet 27 gives its perihelion distance 3⅗. I say, therefore, that the aphelion distance of the former planet is to the perihelion of this as 18 is to 3⅗. From which it is clear that, the harmonies between the extreme motions of two planets having been found, and the periodic times assigned to each, there must result the extreme and mean distances, and, therefore, also the eccentricities.

Twelfthly, it is given also, from different extreme motions of one and the same planet, to find the mean motion. For this is not exactly the arithmetical mean between the extreme motions, nor is it exactly the geometrical mean, but it is as much less than the geometrical mean as the geometrical mean is less than the [arithmetical] mean between the two. Let the two extreme motions be 8 and 10. The mean motion will be less than 9, less even than the root of 80 by a half of the difference between the two, 9 and the root of 80. So, if the aphelion is 20, and the perihelion 24, the mean motion will be less than 22, less even than the root of 480 by a half of the difference between this root and 22.

GALILEO[1]

The Foundation of Telescopic Astronomy

(From "The Sidereal Messenger," by Galileo Galilei, 1610; translated by E. S Carlos, 1880.)

Introduction.—In the present small treatise I set forth some matters of great interest for all observers of natural phenomena to look at and consider. They are of great interest, I think, first, from their intrinsic excellence; secondly, from their absolute novelty; and lastly, also on account of the instrument by the aid of which they have been presented to my apprehension.

The number of the Fixed Stars which observers have been able to see without artificial powers of sight up to this day can be counted. It is therefore decidedly a great feat to add to their number, and to set distinctly before the eyes other stars in myriads, which have never been seen before, and which surpass the old, previously known, stars in number more than ten times.

Again, it is a most beautiful and delightful sight to behold the body of the Moon, which is distant from us nearly sixty semidiameters of the Earth, as near as if it was at a distance of only two of the same measures; so that the diameter of this same Moon appears about thirty times larger, its surface about nine hundred times, and its solid mass nearly 27,000 times larger than when it is viewed only with the naked eye: and consequently any one may know with the certainty that is due to the use of our senses, that the Moon certainly does not possess a smooth and polished surface, but one rough and uneven, and, just like the face of the Earth itself, is everywhere full of vast protuberances, deep chasms, and sinuosities.

[1] Galileo Galilei (1564–1642), the greatest of Italian astronomers, the master pioneer in scientific method and observation. His telescopic discoveries furnished proofs for the Copernican theory and tended seriously to discredit the infallibility of Aristotle and Ptolemy. The "System of the World," from which the quotation on p. 52 is taken, is not of so great scientific importance as his other writings, but it has a reputation as a piece of brilliant controversial writing.

Then to have got rid of disputes about the Galaxy or Milky Way, and to have made its nature clear to the very senses, not to say to the understanding, seems by no means a matter which ought to be considered of slight importance. In addition to this, to point out, as with one's finger, the nature of those stars which every one of the astronomers up to this time has called *nebulous*, and to demonstrate that it is very different from what has hitherto been believed, will be pleasant, and very fine. But that which will excite the greatest astonishment by far; and which indeed especially moved me to call the attention of all astronomers and philosophers, is this, namely, that I have discovered four planets, neither known nor observed by any one of the astronomers before my time, which have their orbits round a certain bright star, one of those previously known, like Venus and Mercury round the Sun, and are sometimes in front of it, sometimes behind it, though they never depart from it beyond certain limits. All which facts were discovered and observed a few days ago by the help of a telescope devised by me, through God's grace first enlightening my mind.

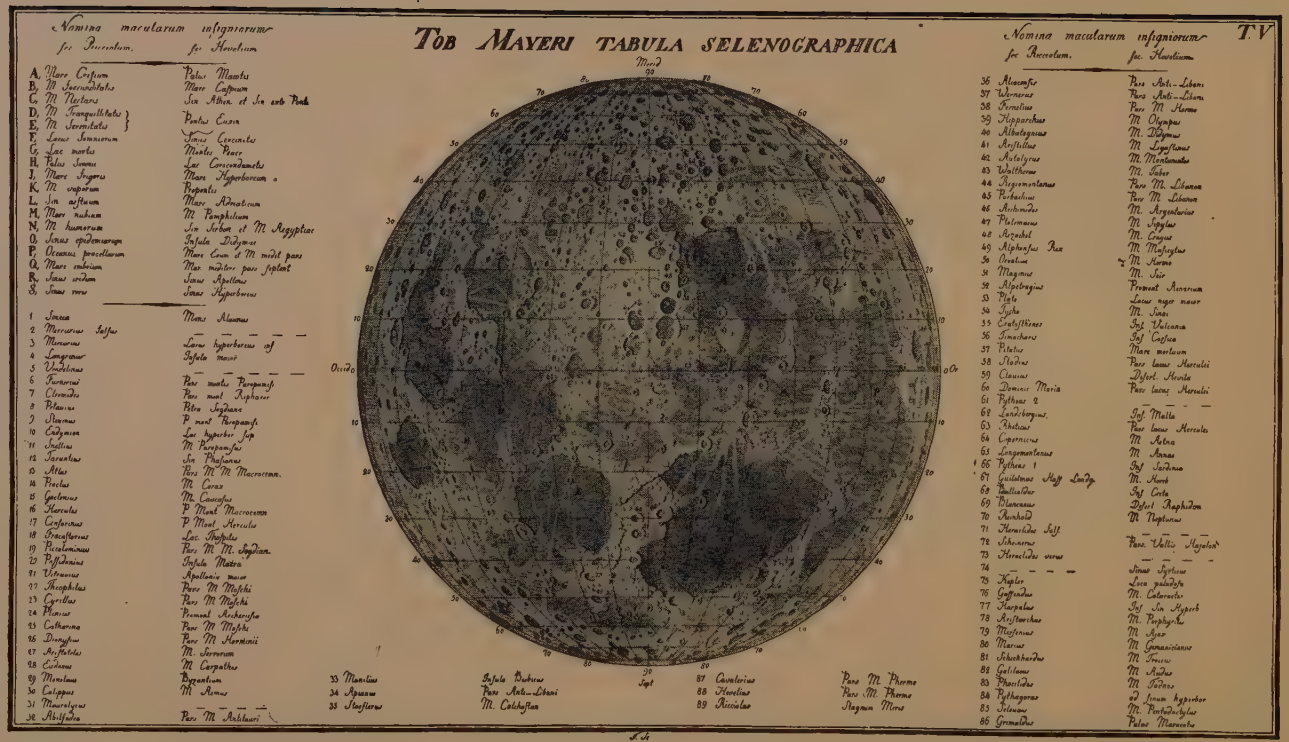
Perchance, other discoveries still more excellent will be made from time to time by me or by other observers, with the assistance of a similar instrument, so I will first briefly record its shape and preparation, as well as the occasion of its being devised, and then I will give an account of the observations made by me.

The Telescope.—About ten months ago a report reached my ears that a Dutchman had constructed a telescope, by the aid of which visible objects, although at a great distance from the eye of the observer, were seen distinctly as if near; and some proofs of its most wonderful performances were reported, which some gave credence to, but others contradicted. A few days after, I received confirmation of the report in a letter written from Paris by a noble Frenchman, Jaques Badovere, which finally determined me to give myself up first to inquire into the principle of the telescope, and then to consider the means by which I might compass the invention of a similar instrument, which after a little while I succeeded in doing, through deep study of the theory of Refraction; and I prepared a tube, at first of lead, in the ends of which I fitted two glass lenses, both plane on one side, but on the other side one spherically convex, and the other concave. Then bringing my eye to the concave lens I saw objects satisfactorily large and near, for they appeared one-third of the distance off and nine times larger than when they are seen with the natural eye alone. I

shortly afterwards constructed another telescope with more nicety, which magnified objects more than sixty times. At length, by sparing neither labour nor expense, I succeeded in constructing for myself an instrument so superior that objects seen through it appear magnified nearly a thousand times, and more than thirty times nearer than if viewed by the natural powers of sight alone.

First Telescopic Observations.—It would be altogether a waste of time to enumerate the number and importance of the benefits which this instrument may be expected to confer, when used by land or sea. But without paying attention to its use for terrestrial objects, I betook myself to observations of the heavenly bodies; and first of all, I viewed the Moon as near as if it was scarcely two semidiameters of the Earth distant. After the Moon, I frequently observed other heavenly bodies, both fixed stars and planets, with incredible delight; and, when I saw their very great number, I began to consider about a method by which I might be able to measure their distances apart, and at length I found one. And here it is fitting that all who intend to turn their attention to observations of this kind should receive certain cautions. For, in the first place, it is absolutely necessary for them to prepare a most perfect telescope, one which will show very bright objects distinct and free from any mistiness, and will magnify them at least 400 times, for then it will show them as if only one-twentieth of their distance off. For, unless the instrument be of such power, it will be in vain to attempt to view all the things which have been seen by me in the heavens, or which will be enumerated hereafter . . .

Observations of Lunar Mountains and Valleys.—Let me first speak of the surface of the Moon, which is turned towards us. For the sake of being understood more easily, I distinguish two parts in it, which I call respectively the brighter and the darker. The brighter part seems to surround and pervade the whole hemisphere; but the darker part, like a sort of cloud, discolours the Moon's surface and makes it appear covered with spots. Now these spots, as they are somewhat dark and of considerable size, are plain to every one, and every age has seen them, wherefore I shall call them *great* or *ancient* spots, to distinguish them from other spots, smaller in size, but so thickly scattered that they sprinkle the whole surface of the Moon, but especially the brighter portion of it. These spots have never been observed by any one before me; and from my observations of them, often repeated, I have been



Topography of the Moon from the *Selenotopographische Fragmente* , 1791, of John Hieronymus Schroeter (1745–1816). His chart is adapted from the earlier work of Tobias Mayer (1723–1762). The names assigned the surface features by John Baptist Riccioli (1598–1671) in his *New Almagest*, 1651, are now in current use. The names assigned by Hevelius (John Hevel, 1611–1687) are given in his *Selenographia*, 1647.

led to that opinion which I have expressed, namely, that I feel sure that the surface of the Moon is not perfectly smooth, free from inequalities and exactly spherical, as a large school of philosophers considers with regard to the Moon and the other heavenly bodies, but that, on the contrary, it is full of inequalities, uneven, full of hollows and protuberances, just like the surface of the Earth itself, which is varied everywhere by lofty mountains and deep valleys.

The appearances from which we may gather these conclusions are of the following nature: On the fourth or fifth day after new-moon, when the Moon presents itself to us with bright horns, the boundary which divides the part in shadow from the enlightened part does not extend continuously in an ellipse, as would happen in the case of a perfectly spherical body, but it is marked out by an irregular, uneven, and very wavy line . . . for several bright excrescences, as they may be called, extend beyond the boundary of light and shadow into the dark part, and on the other hand pieces of shadow encroach upon the light—nay, even a great quantity of small blackish spots, altogether separated from the dark part, sprinkle everywhere almost the whole space which is at the time flooded with the Sun's light, with the exception of that part alone which is occupied by the great and ancient spots. I have noticed that the small spots just mentioned have this common characteristic always and in every case, that they have the dark part towards the Sun's position, and on the side away from the Sun they have brighter boundaries, as if they were crowned with shining summits. Now we have an appearance quite similar on the Earth about sunrise, when we behold the valleys, not yet flooded with light, but the mountains surrounding them on the side opposite to the Sun already ablaze with the splendour of his beams; and just as the shadows in the hollows of the Earth diminish in size as the Sun rises higher, so also these spots on the Moon lose their blackness as the illuminated part grows larger and larger. Again, not only are the boundaries of light and shadow in the Moon seen to be uneven and sinuous, but—and this produces still greater astonishment—there appear very many bright points within the darkened portion of the Moon, altogether divided and broken off from the illuminated tract, and separated from it by no inconsiderable interval, which, after a little while, gradually increase in size and brightness, and after an hour or two become joined on to the rest of the main portion, now become somewhat larger; but in the

meantime others, one here and another there, shooting up as if growing, are lighted up within the shaded portion, increase in size, and at last are linked on to the same luminous surface, now still more extended . . . Now, is it not the case on the Earth before sunrise, that while the level plain is still in shadow, the peaks of the most lofty mountains are illuminated by the Sun's rays? After a little while does not the light spread further, while the middle and larger parts of those mountains are becoming illuminated; and at length, when the Sun has risen, do not the illuminated parts of the plains and hills join together? The grandeur, however, of such prominences and depressions in the Moon seems to surpass both in magnitude and extent the ruggedness of the Earth's surface, as I shall hereafter show . . .

Appearance of Stars in the Telescope.—Hitherto I have spoken of the observations which I have made concerning the Moon's body; now I will briefly announce the phenomena which have been, as yet, seen by me with reference to the Fixed Stars. And first of all the following fact is worthy of consideration: The stars, fixed as well as erratic, when seen with a telescope, by no means appear to be increased in magnitude in the same proportion as other objects, and the Moon herself, gain increase of size; but in the case of the stars such an increase appears much less, so that you may consider that a telescope, which (for the sake of illustration) is powerful enough to magnify other objects a hundred times, will scarcely render the stars magnified four or five times. But the reason of this is as follows: When stars are viewed with our natural eyesight they do not present themselves to us of their bare, real size, but beaming with a certain vividness, and fringed with sparkling rays, especially when the night is far advanced; and from this circumstance they appear much larger than they would if they were stripped of those adventitious fringes, for the angle which they subtend at the eye is determined not by the primary disc of the star, but by the brightness which so widely surrounds it . . . A telescope . . . removes from the stars their adventitious and accidental splendours before it enlarges their true discs (if indeed they are of that shape), and so they seem less magnified than other objects, for a star of the fifth or sixth magnitude seen through a telescope is shown as of the first magnitude only.

The difference between the appearance of the planets and the fixed stars seems also deserving of notice. The planets present their discs perfectly round, just as if described with a pair of

compasses, and appear as so many little moons, completely illuminated and of a globular shape; but the fixed stars do not look to the naked eye bounded by a circular circumference, but rather like blazes of light, shooting out beams on all sides and very sparkling, and with a telescope they appear of the same shape as when they are viewed by simply looking at them, but so much larger that a star of the fifth or sixth magnitude seems to equal Sirius, the largest of all the fixed stars.

The Infinite Multitude of Telescopic Stars.—But beyond the stars of the sixth magnitude you will behold through the telescope a host of other stars, which escape the unassisted sight, so numerous as to be almost beyond belief, for you may see more than six other differences of magnitude, and the largest of these, which I may call stars of the seventh magnitude, or of the first magnitude of invisible stars, appear with the aid of the telescope larger and brighter than stars of the second magnitude seen with the unassisted sight. But in order that you may see one or two proofs of the inconceivable manner in which they are crowded together, I have determined to make out a case against two star-clusters, that from them as a specimen you may decide about the rest.

As my first example, I had determined to depict the entire constellation of Orion, but I was overwhelmed by the vast quantity of stars and by want of time, and so I have deferred attempting this to another occasion, for there are adjacent to, or scattered among, the old stars more than five hundred new stars within the limits of one or two degrees. For this reason I have selected the three stars in Orion's Belt and the six in his Sword, which have been long well-known groups, and I have added eighty other stars recently discovered in their vicinity, and I have preserved as exactly as possible the intervals between them. The well-known or old stars, for the sake of distinction, I have depicted of larger size, and I have outlined them with a double line; the others, invisible to the naked eye, I have marked smaller and with one line only. I have also preserved the differences of magnitude as much as I could. As a second example, I have depicted the six stars of the constellation Taurus, called the Pleiades (I say *six* intentionally, since the seventh is scarcely ever visible), a group of stars which is enclosed in the heavens within very narrow precincts. Near these there lie more than forty others invisible to the naked eye, no one of which is more than half a degree off any of the aforesaid six; of these I have noticed only thirty-six in

my diagram. I have preserved their intervals, magnitudes, and the distinction between the old and the new stars, just as in the case of the constellation Orion.

Orion's belt and sword.

Pleiades.

Telescopic Appearance of Milky Way.—The next object which I have observed is the essence or substance of the Milky Way. By the aid of a telescope any one may behold this in a manner which so distinctly appeals to the senses that all the disputes which have tormented philosophers through so many ages are exploded at

once by the irrefragable evidence of our eyes, and we are freed from wordy disputes upon this subject, for the Galaxy is nothing else but a mass of innumerable stars planted together in clusters. Upon whatever part of it you direct the telescope straightway a vast crowd of stars presents itself to view; many of them are tolerably large and extremely bright, but the number of small ones is quite beyond determination . . .

Discovery of Jupiter's Satellites.—I have now finished my brief account of the observations which I have thus far made with regard to the Moon, the Fixed Stars, and the Galaxy. There remains the matter, which seems to me to deserve to be considered the most important in this work, namely, that I should disclose and publish to the world the occasion of discovering and observing four Planets, never seen from the very beginning of the world up to our own times, their positions, and the observations made during the last two months about their movements and their changes of magnitude; and I summon all astronomers to apply themselves to examine and determine their periodic times, which it has not been permitted me to achieve up to this day, owing to the restriction of my time. I give them warning, however, again, so that they may not approach such an inquiry to no purpose, that they will want a very accurate telescope, and such as I have described in the beginning of this account.

On the 7th day of January in the present year, 1610, in the first hour of the following night, when I was viewing the constellations of the heavens through a telescope, the planet Jupiter presented itself to my view, and as I had prepared for myself a very excellent instrument, I noticed a circumstance which I had never been able to notice before, owing to want of power in my other telescope, namely, that three little stars, small but very bright, were near the planet; and although I believed them to belong to the number of the fixed stars, yet they made me somewhat wonder, because they seemed to be arranged exactly in a straight line, parallel to the ecliptic, and to be brighter than the rest of the stars, equal to them in magnitude. The position of them with reference to one another and to Jupiter was as follows:

Ori. * * O * *Occ.*

On the east side there were two stars, and a single one towards the west. The star which was furthest towards the east, and the western star, appeared rather larger than the third.

I scarcely troubled at all about the distance between them and Jupiter, for, as I have already said, at first I believed them to be fixed stars; but when on January 8th, led by some fatality, I turned again to look at the same part of the heavens, I found a very different state of things, for there were three little stars all west of Jupiter, and nearer together than on the previous night, and they were separated from one another by equal intervals, as the accompanying figure shows.

Ori. O * * * *Occ.*

At this point, although I had not turned my thoughts at all upon the approximation of the stars to one another, yet my surprise began to be excited, how Jupiter could one day be found to the east of all the aforesaid fixed stars when the day before it had been west of two of them; and forthwith I became afraid lest the planet might have moved differently from the calculation of astronomers, and so had passed those stars by its own proper motion. I, therefore, waited for the next night with the most intense longing, but I was disappointed of my hope, for the sky was covered with clouds in every direction.

But on January 10th the stars appeared in the following position with regard to Jupiter, the third, as I thought, being

Ori. * * O *Occ.*

hidden by the planet. They were situated just as before, exactly in the same straight line with Jupiter, and along the Zodiac . . .

When I had seen these phenomena, as I knew that corresponding changes of position could not by any means belong to Jupiter, and as, moreover, I perceived that the stars which I saw had always been the same, for there were no others either in front or behind, within a great distance, along the Zodiac—at length, changing from doubt into surprise, I discovered that the interchange of position which I saw belonged not to Jupiter, but to the stars to which my attention had been drawn, and I thought therefore that they ought to be observed henceforward with more attention and precision.

Accordingly, on January 11th I saw an arrangement of the following kind:

Ori. * * O *Occ.*

namely, only two stars to the east of Jupiter, the nearer of which was distant from Jupiter three times as far as from the star further

to the east; and the star furthest to the east was nearly twice as large as the other one; whereas on the previous night they had appeared nearly of equal magnitude. I, therefore, concluded, and decided unhesitatingly, that there are three stars in the heavens moving about Jupiter, as Venus and Mercury round the Sun; which at length was established as clear as daylight by numerous other subsequent observations. These observations also established that there are not only three, but four, erratic sidereal bodies performing their revolutions round Jupiter . . .

These are my observations upon the four Medicean planets, recently discovered for the first time by me; and although it is not yet permitted me to deduce by calculation from these observations the orbits of these bodies, yet I may be allowed to make some statements, based upon them, well worthy of attention.

Orbits and Periods of Jupiter's Satellites.—And, in the first place, since they are sometimes behind, sometimes before Jupiter, at like distances, and withdraw from this planet towards the east and towards the west only within very narrow limits of divergence, and since they accompany this planet alike when its motion is retrograde and direct, it can be a matter of doubt to no one that they perform their revolutions about this planet, while at the same time they all accomplish together orbits of twelve years' length about the centre of the world. Moreover, they revolve in unequal circles, which is evidently the conclusion to be drawn from the fact that I have never been permitted to see two satellites in conjunction when their distance from Jupiter was great, whereas near Jupiter two, three, and sometimes all four, have been found closely packed together. Moreover, it may be detected that the revolutions of the satellites which describe the smallest circles round Jupiter are the most rapid, for the satellites nearest to Jupiter are often to be seen in the east, when the day before they have appeared in the west, and contrariwise. Also, the satellite moving in the greatest orbit seems to me, after carefully weighing the occasions of its returning to positions previously noticed, to have a periodic time of half a month. Besides, we have a notable and splendid argument to remove the scruples of those who can tolerate the revolution of the planets round the Sun in the Copernican system, yet are so disturbed by the motion of one Moon about the Earth, while both accomplish an orbit of a year's length about the Sun, that they consider that this theory of the universe must be upset as impossible: for now we have not one planet only revolv-

ing about another, while both traverse a vast orbit about the Sun, but our sense of sight presents to us four satellites circling about Jupiter, like the Moon about the Earth, while the whole system travels over a mighty orbit about the Sun in the space of twelve years.

The Ptolemaic and Copernican Systems

(From the "System of the World," Mathematical Collections and Translations, Salusbury, 1661.)

There was published some years since in Rome a salutiferous Edict, that, for the obviating of the dangerous Scandals of the present Age, imposed a seasonable Silence upon the Pythagorean Opinion of the Mobility of the Earth. There want not such as unadvisedly affirm, that the Decree was not the production of a sober Scrutiny, but of an ill informed Passion; and one may hear some mutter that Consultors altogether ignorant of Astronomical Observations ought not to clip the Wings of Speculative Wits with rash Prohibitions. My zeale cannot keep silence when I hear these inconsiderate complaints. I thought fit, as being thoroughly acquainted with that prudent Determination, to appear openly upon the Theatre of the World as a Witness of the naked Truth. I was at that time in Rome; and had not only the audiences, but applauds of the most Eminent Prelates of that Court; nor was that Decree Published without Previous Notice given me thereof. Therefore, it is my resolution in the present case to give Foreign Nations to see, that this point is as well understood in Italy, and particularly in Rome, as Transalpine Diligence can imagine it to be: and collecting together all the proper Speculations that concern the Copernican Systeme, to let them know, that the notice of all preceded the Censure of the Roman Court; and that there proceed from this Climate not only Doctrines for the health of the Soul, but also ingenious Discoveries for the recreating of the Mind.

To this end I have personated the Copernican in this Discourse; proceeding upon an Hypothesis purely Mathematical; striving by all artificial wayes to represent it Superiour, not to that of the Immobility of the Earth absolutely, but according as it is mentioned by some, that retain no more, but the name of Peripateticks, and are content, without going farther, to adore shadows, not

philosophizing with requisit caution, but with the sole remembrance of four Principles, but badly understood.

We shall treat of three principall heads. First I will endeavor to show that all Experiments that can be made upon the Earth are insufficient means to conclude its Mobility, but are indifferently applicable to the Earth moveable or immoveable: and I hope that on this occasion we shall discover many observable passages unknown to the Ancients. Secondly, we will examine the Cœlestiall Phœnomena that make for the Copernican Hypothesis, as if it were to prove absolutely victorious; adding by the way certain new observations, which yet serve only for the Astronomical Facility, not for Natural Necessity. In the third place I will propose an ingenuous Fancy. I remember that I have said many years since, that the unknown Probleme of the Tide might receive some light, admitting the Earth's Motion. This Position of mine passing from one to another had found charitable Fathers that adopted it for the issue of their own wit. Now, because no stranger may ever appear that defending himself with our armes, shall charge us with want of caution in so principal an Accident, I have thought good to lay down those probabilities that would render it credible, admitting that the Earth did move. I hope, that by these Considerations the World will come to know, that if other Nations have Navigated more than we, we have not studied less than they; and that our returning to assert the Earth's Stability, and to take the contrary only for a Mathematical Capriccio, proceeds not from inadvertency of what others have thought thereof, but (had we no other inducements) from those Reasons that Piety, Religion, the Knowledge of the Divine Omnipotency, and consciousness of the incapacity of man's Understanding dictate unto us . . .

Salviatus. You argue very well; but you know that the principal scope of *Astronomers*, is to render only reason for the appearances in the Cælestial Bodies, and to them, and to the motions of the Stars, to accommodate such structures and compositions of Circles, that the motions following those calculations, answer to the said appearances, little scrupling to admit of some exorbitances, that indeed upon other accounts they would much stick at. And *Copernicus* himself writes, that he had in his first studies restored the Science of *Astronomy* upon the very suppositions of *Ptolomy*, and in such manner corrected the motions of the Planets, that the computations did very exactly agree with the

Phænomena, and the *Phænomena* with the supputations, in case that he took the Planets severally one by one. But he addeth, that in going about to put together all the structures of the particular Fabricks, there resulted thence a Monster and *Chimæra*, composed of members most disproportionate to one another, and altogether incompatible; so that although it satisfied an *Astronomer* merely *Arithmetical*, yet did it not afford satisfaction or content to the *Astronomer Phylosophical*. And because he very well understood, that if one might salve the Cælestial appearances with false assumptions in nature, it might with much more ease be done by true suppositions, he set himself diligently to search whether any amongst the ancient men of fame, had ascribed to the World any other structure, than that commonly received by *Ptolomy;* and finding that some *Pythagoreans* had in particular assigned the Diurnal conversion to the Earth, and others the annual motion also, he began to compare the appearances, and particularities of the Planets motions, with these two new suppositions, all which things jump exactly with his purpose; and seeing the whole correspond, with admirable facility to its parts, he imbraced this new Systeme, and it took up his rest.

SIMPLICIUS. But what great exorbitancies are there in the *Ptolomaick* Systeme, for which there are not greater to be found in this of *Copernicus?*

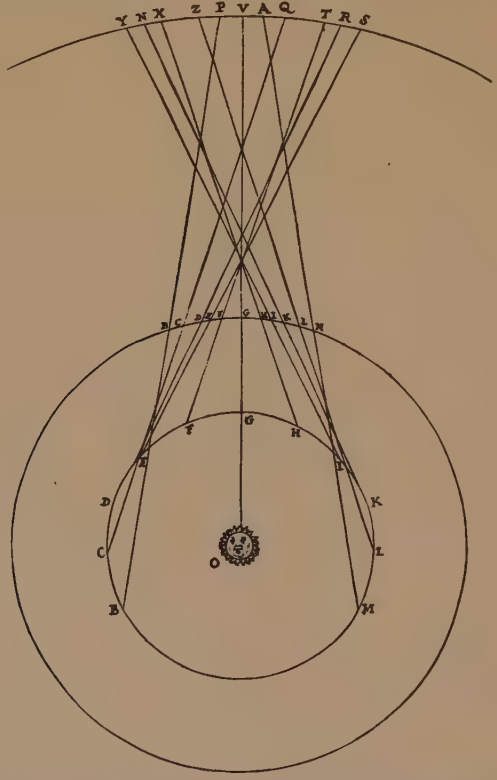
SALVIATUS. In the *Ptolomaick Hypothesis* there are diseases, and in the *Copernican* their cures. And first will not all the Sects of *Phylosophers*, account it a great inconvenience, that a body naturally moveable in circumgyration, should move irregularly upon its own Centre, and regularly upon another point? And yet there are such deformed motions as these in the *Ptolomaean* Hypothesis, but in the *Copernican* all move evenly about their own Centres. In the *Ptolomaick*, it is necessary to assign to the Cælestial bodies, contrary motions, and to make them all to move, from East to West, and at the same time, from West to East; but in the *Copernican*, all the Cælestial revolutions are towards one onely way, from West to East. But what shall we say of the apparent motion of the Planets, so irregular, that they not only go one while swift, and another while slow, but sometimes wholly seace to move; and then after a long time return back again? To salve which appearances *Ptolomie* introduceth very great *Epicicles*, accommodating them one by one to each Planet, with some rules of incongruous motions, which are all with one single motion of the

Earth taken away. And would not you, *Simplicius*, call it a great absurditie, if in the *Ptolomaick* Hypothesis, in which the particular Planets, have their peculiar Orbs assigned them one above another, one must be frequently forced to say, that *Mars*, constituted above the Sphære of the Sun, doth so descend, that breaking the Solar Orb, it goeth under it, and approacheth nearer to the Earth, than to the Body of the Sun, and by and by immeasurably ascendeth above the same? And yet this, and other exorbitancies are remedied by the sole and single annual motion of the Earth.

SAGREDUS. I would gladly be better informed how these stations, and retrograde and direct motions, which did ever seem to me great improbabilities, do accord in this *Copernican* Systeme.

SALVIATUS. You shall see them so to accord, *Sagredus*, that this onely conjecture ought to be sufficient to make one that is not more than pertinacious or stupid, yield, assent to all the rest of this Doctrine. I tell you therefore, that nothing being altered in the motion of *Saturn*, which is 30 years, in that of *Jupiter*, which is 12, in that of *Mars*, which is 2, in that of *Venus*, which is 9 moneths, in that of *Mercury*, which is 80 dayes, or thereabouts, the sole annual motion of the Earth between *Mars* and *Venus*, causeth the apparent inequalities in all the five stars before named. And for a facile and full understanding of the whole, I will describe this figure of it. Therefore suppose the Sun to be placed in the centre O, about which we will draw the Orb described by the Earth, with the annual motion BGM, and let the circle described, *v. gr.* by *Jupiter* about the Sun in 12 years, be this BGM, and in the starry sphere let us imagine the Zodiack YVS. Again, in the annual Orb of the Earth let us take certain equal arches, BC, CD, EF, FG, GH, HI, IK, KL, LM, and in the Sphere of *Jupiter* let us make certain other arches, passed in the same times in which the Earth passeth hers, which let be BC, CD, DE, EF, FG, GH, HI, IK, KL, LM, which shall each be proportionally lesse than these marked in the Earths Orb, like as the motion of *Jupiter* under the Zodiack is slower than the annual. Supposing now, that when the Earth is in B, *Jupiter* is in B, it shall appear to us in the Zodiack to be in P, describing the right line BBP. Next suppose the Earth to be moved from B to C, and *Jupiter* from B to C, in the same time; *Jupiter* shall appear to have passed in the Zodiack to Q, and to have moved straight forwards, according to the order of the signes PQ. In the next place, the

Earth passing to D, and *Jupiter* to D, it shall be seen in the Zodiack in R, and from E, *Jupiter* being come to E; will appear in the Zodiack in S, having all this while moved right forwards. But the Earth afterwards beginning to interpose more directly between *Jupiter* and the Sun, she being come to F, and *Jupiter* to F, he will appear in T, to have already begun to return apparently back again under the Zodiack, and in that time that the Earth shall



have passed the arch EF, *Jupiter* shall have entertained himself between the points ST, and shall have appeared to us almost motionlesse and stationary. The Earth being afterwards come to G, and *Jupiter* to G, in opposition to the Sun, it shall be visible in the Zodiack at V, and much returned backwards by all the arch of the Zodiack TV; howbeit that all the way pursuing its even course it hath really gone forwards not onely in its own circle, but in the Zodiack also in respect to the center of the said Zodiack, and to the Sun placed in the same. The Earth and *Jupiter* again con-

tinuing their motions, when the Earth is come to H, and *Jupiter* to H, it shall seem very much gone backward in the Zodiack by all the arch VX. The Earth being come to I, and *Jupiter* to I, it shall be apparently moved in the Zodiack by the little space XY, and there it will seem stationary. When afterwards the Earth shall be come to K, and *Jupiter* to K; in the Zodiack he shall have passed the arch YN in a direct motion; and the Earth pursuing its course to L, shall see *Jupiter* in L, in the point Z. And lastly *Jupiter* in M shall be seen from the Earth M, to have passed to A, with a motion still right forwards; and its whole apparent retrogradation in the Zodiack shall answer to the arch SY, made by *Jupiter*, whilst that he in his own circle passeth the arch EI, and the Earth in hers the arch EI. And this which hath been said, is intended of *Saturn* and of *Mars* also; and in *Saturn* those retrogradations are somewhat more frequent than in *Jupiter*, by reason that its motion is a little slower than that of *Jupiter*, so that the Earth overtaketh it in a shorter space of time; in *Mars* again they are more rare, for that its motion is more swift than that of *Jupiter*. Whereupon the Earth consumeth more time in recovering it. Next as to *Venus* and *Mercury*, whose Circles are comprehended by that of the Earth, their stations and regressions appear to be occasioned, not by their motions that really are such, but by the anual motion of the said Earth, as *Copernicus* excellently demonstrateth.

HORROX[1]

The First Observation of a Transit of Venus

(From "The Transit of Venus across the Sun," 1639; translated by Rev. A. B. Whatton, 1859.)

Soon after the commencement of my astronomical studies, and whilst preparing for practical observation, I computed the Ephemerides of several years, from the continuous tables of Lansberg. Having followed up the task with unceasing perseverance, and having arrived at the point of its completion, the very erroneous calculation of these tables, then detected, convinced me that an astronomer might be engaged upon a better work. Accordingly I broke off the useless computation, and resolved for the future with my own eyes to observe the positions of the stars in the heavens; but lest so many hours spent on Lansberg should be entirely thrown away, I made use of my Ephemerides in ascertaining the positions of the distant planets, so that I was enabled to predict their conjunctions, their appulses to the fixed stars, and many other extraordinary phenomena. Delighted for the time with such a foretaste of the science, I took great pains carefully to prepare myself for further observation.

Whilst thus engaged, I received my first intimation of this remarkable conjunction of Venus with the Sun; and I regard it as a very fortunate occurrence, inasmuch as about the beginning of October, 1639, it induced me, in expectation of so grand a spectacle, to observe with increased attention . . .

[1] Jeremiah Horrox (1619–1641), a devout English clergyman, who arrived at distinction in intellectual pursuits during his short life of twenty-two years. "He was the first to predict and observe the transit of Venus in 1639; to reduce the Sun's parallax nearly to what it has since been determined; to discover the orbit of the moon to be an ellipse about the earth . . . ; to devise the beautiful experiment of the circular pendulum for illustrating the action of a central force; and to commence a regular series of tidal observations for the purpose of philosophical enquiry: besides all which, he effected improvements in different astronomical tables, recommended the adoption of decimal notation, detected the inequality in the mean motion of Jupiter and Saturn, and wrote his opinions upon the nature and movements of comets" (Whatton).

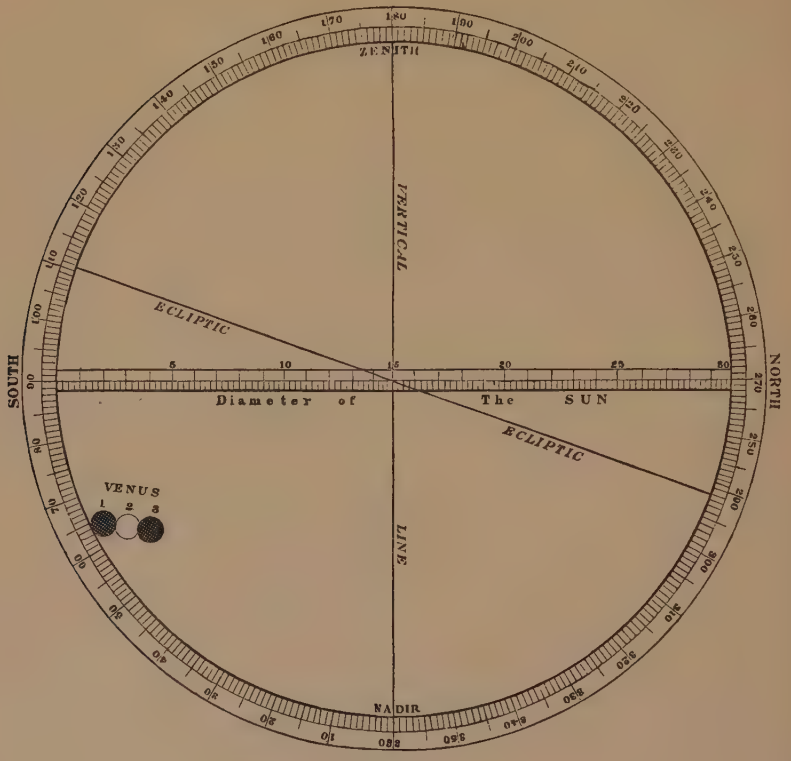
The more accurate calculations of Rudolphi very much confirmed my expectations; and I rejoiced exceedingly in the hope of seeing Venus, the rarity of whose appearance in conjunction with the Sun had induced me to pay less attention to the more common phenomena of the same kind visible in the planet Mercury; for though hitherto these phenomena have been observed on one occasion only, the science of astronomy holds out to us the assurance that they will, even in our time, frequently appear.

But lest a vain exultation should deceive me, and to prevent the chance of disappointment, I not only determined diligently to watch the important spectacle myself, but exhorted others whom I knew to be fond of astronomy to follow my example; in order that the testimony of several persons, if it should so happen, might the more effectually promote the attainment of truth; and because by observing in different places, our purpose would be less likely to be defeated by the accidental interposition of the clouds or any fortuitous impediment.

The chance of a clouded atmosphere caused me much anxiety; for Jupiter and Mercury were in conjunction with the Sun almost at the same time as Venus. This remarkable assemblage of the planets (as if they were desirous of beholding, in common with ourselves, the wonders of the heavens, and of adding to the splendour of the scene), seemed to forebode great severity of weather. Mercury, whose conjunction with the Sun is invariably attended with storm and tempest, was especially to be feared. In this apprehension I coincide with the opinion of the astrologers, because it is confirmed by experience; but in other respects I cannot help despising their more than puerile vanities . . .

Having attentively examined Venus with my instrument, I described on a sheet of paper a circle whose diameter was nearly equal to six inches, the narrowness of the apartment not permitting me conveniently to use a larger size. This however admitted of a sufficiently accurate division; nor could the arc of a quadrant be apportioned more exactly, even with a radius of fifty feet, which is as great an one as any astronomer has divided; and it is in my opinion far more convenient than a larger, for although it represents the sun's image less, yet it depicts it more clearly and steadily. I divided the circumference of this circle into 360° in the usual manner, and its diameter into thirty equal parts, which gives about as many minutes as are equivalent to the sun's apparent diameter; each of these thirty parts was again divided into four equal por-

tions, making in all one hundred and twenty; and these, if necessary, may be more minutely subdivided; the rest I left to ocular computation, which, in such small sections, is quite as certain as any mechanical division. Suppose then each of these thirty parts to be divided into 60″, according to the practice of astronomers. When the time of the observation approached, I retired to my



Venus on the sun's disc.

apartment, and having closed the windows against the light, I directed my telescope, previously adjusted to a focus, through the aperture towards the sun and received his rays at right angles upon the paper already mentioned. The sun's image exactly filled the circle, and I watched carefully and unceasingly for any dark body that might enter upon the disc of light.

Although the corrected computation of Venus' motions which I had before prepared, and on the accuracy of which I implicitly

relied, forbad me to expect anything before three o'clock in the afternoon of the 24th; yet since, according to the calculations of most astronomers, the conjunction should take place sooner, by some even on the 23rd, I was unwilling to depend entirely on my own opinion which was not sufficiently confirmed, lest by too much self-confidence I might endanger the observation. Anxiously intent, therefore, on the undertaking through the greater part of the 23rd, and the whole of the 24th, I omitted no available opportunity of observing her ingress. I watched carefully on the 24th from sunrise to nine o'clock, and from a little before ten until noon, and at one in the afternoon, being called away in the intervals by business of the highest importance which, for these ornamental pursuits, I could not with propriety neglect. But during all this time I saw nothing in the sun except a small and common spot, consisting as it were of three points at a distance from the centre towards the left, which I noticed on the preceding and following days. This evidently had nothing to do with Venus. About fifteen minutes past three in the afternoon, when I was again at liberty to continue my labors, the clouds, as if by divine interposition, were entirely dispersed, and I was once more invited to the grateful task of repeating my observations. I then beheld a most agreeable spectacle, the object of my sanguine wishes, a spot of unusual magnitude and of a perfectly circular shape, which had already fully entered upon the sun's disc on the left, so that the limbs of the Sun and Venus precisely coincided, forming an angle of contact. Not doubting that this was really the shadow of the planet, I immediately applied myself sedulously to observe it.

In the first place, with respect to the inclination, the line of the diameter of the circle being perpendicular to the horizon, although its plane was somewhat inclined on account of the Sun's altitude, I found that the shadow of Venus at the aforesaid hour, namely fifteen minutes past three, had entered the Sun's disc about 62°30′, certainly between 60° and 65°, from the top towards the right. This was the appearance in the dark apartment; therefore out of doors beneath the open sky, according to the law of optics, the contrary would be the case, and Venus would be below the centre of the sun, distant 62°30′ from the lower limb, or the nadir, as the Arabians term it. The inclination remained to all appearance the same until sunset, when the observation was concluded.

In the second place, the distance between the centres of Venus and the Sun I found, by three observations, to be as follows:

The Hour	Distance of the Centres
At 3:15 by the clock	14′24″
At 3:35 by the clock	13′30″
At 3:45 by the clock	13′0″
At 3:50 the apparent sunset.	

The true setting being 3:45, and the apparent about five minutes later, the difference being caused by refraction. The clock therefore was sufficiently correct.

In the third place, I found after careful and repeated observation, that the diameter of Venus, as her shadow was depicted on the paper, was larger indeed than the thirtieth part of the solar diameter, though not more so than the sixth, or at the utmost the fifth, of such a part. Therefore let the diameter of the Sun be to the diameter of Venus as 30′ to 1′12″. Certainly her diameter never equalled 1′30″, scarcely perhaps 1′20″, and this was evident as well when the planet was near the Sun's limb, as when far distant from it.

This observation was made in an obscure village where I have long been in the habit of observing, about fifteen miles to the north of Liverpool, the latitude of which I believe to be 53°20′, although by the common maps it is stated at 54°12′, therefore the latitude of the village will be 53°35′, and the longitude of both 22°30′ from the Fortunate Islands, now called the Canaries. This is 14°15′ to the west of Uraniburg in Denmark, the longitude of which is stated by Brahe, a native of the place, to be 36°45′ from these Islands.

This is all I could observe respecting this celebrated conjunction, during the short time the Sun remained in the horizon: for although Venus continued on his disc for several hours, she was not visible to me longer than half-an-hour, on account of his so quickly setting. Nevertheless, all the observations which could possibly be made in so short a time, I was enabled, by Divine Providence, to complete so effectually that I could scarcely have wished for a more extended period. The inclination was the only point upon which I failed to attain the utmost precision; for, owing to the rapid motion of the Sun, it was difficult to observe with certainty to a single degree, and I frankly confess, that I neither did nor could ascertain it. But all the rest is sufficiently accurate, and as exact as I could desire.

HUYGENS[1]

Saturn's Ring

(From "Systema Saturnium," 1659; translation by Dr. John H. Walden, 1928.)

When Galileo made use of the telescope, noblest invention of our Belgic nation, for observation of the heavenly bodies, and, before all other men, disclosed to mortals those very celebrated phenomena of the planets, the most wonderful of his discoveries, it would seem, were those relating to the star of Saturn. For all the other phenomena, though justly calling for our wonder and admiration, were still not of a kind to make it necessary to question strongly the causes of their existence. But Saturn's changing forms showed a new and strange device of nature, the principle of which neither Galileo himself nor, in all the time since, any of the astronomers (with their permission be it said) has succeeded in divining. Galileo had first seen this star shining, not as a single orb, but in what seemed to be a triple form, as two smaller stars in close proximity to, and on opposite sides of, a larger star, in line with its center. And seeing this form continue for nearly three years with no change, he had become firmly convinced that, just as Jupiter was provided with four satellites, so Saturn was provided with two, which, however, had no motion, and so would always cling to the sides of Saturn in the same position. But when Saturn came forth alone, quite destitute of his former retinue of satellites, Galileo was obliged to change his opinion. Astonished by what he saw, he tried to reach by conjecture the cause of the appearance, and made a few predictions as to the time when the former phase was due to recur. But, it was shown by the event, these predictions

[1] Christiaan Huygens (1629–1695), famous Dutch astronomer, developed the art of grinding lenses, and through his telescope discovered Saturn's satellite Titan. By keen interpretation of his observations, he deduced the existence of the ring around Saturn and found its inclination to the ecliptic. His rediscovery of the pendulum-clock revolutionized the art of exact astronomical observation; important theoretical results are worked out in the classical publication "Horologium Oscillatorium" (1673). His proofs of the fundamental laws of optics were published in 1690.

were not then fulfilled according to his expectation, nor, it appeared, was Saturn satisfied with having only two aspects. For a succession of other strange and marvelous forms was revealed, which I find first described by Josephus Blancanus and Franciscus Fontana—forms of such unusual appearance that they were considered by many as a mockery of the eyes, shapes adhering to the lenses rather than existing in the heavens; but after the same forms had been seen by more, it became clear that it was no false evidence that revealed them.

And so I was also drawn by an urgent longing to behold these wonders of the heaven. But I had only the ordinary form of telescope, which measured five or six feet in length. I, therefore, set myself to work with all the earnestness and seriousness I could command to learn the art by which glasses are fashioned for these uses, and I did not regret having put my own hand to the task. After overcoming great difficulties (for this art has in reserve more difficulties than it seems to bear on its face), I at last succeeded in making the lenses which have provided me with the material for writing this account. For upon immediately directing my telescope at Saturn, I found that things there had quite a different appearance from that which they had previously been thought by most men to have. For it appeared that the two neighboring appendages clinging to Saturn were by no means two planets, but rather something different, while, distinct from these, there was a single planet, at a greater distance from Saturn and revolving around him in sixteen days; and the existence of this planet had been unknown through all the centuries up to that time. Following the wise advice of a distinguished man, one equally conspicuous for his ability and his character, Johannes Capellanus, I, three years ago informed astronomers of my new observation. For while I was sojourning at Paris, I told Capellanus, as well as Gassendi and others, of the satellite of Saturn which I had seen, and Capellanus gave me many reasons for believing that I ought not to withhold an announcement that would be so pleasing to all men until I should finish the work on the complete System of Saturn, which I was then engaged upon. And so, on the 5th day of March,[1] in the year 1656, I put forth the result of my observation on the Moon of Saturn (for so I have quite properly named the new star), and, together with it, an hypothesis containing an explanation of the other phenomena of Saturn; in the case

[1 Later Huygens says the twenty-fifth of March (see p. 68).]

of the latter, however, I confused the order of the letters in which it was written, that it might witness to the fact simply that I was not unacquainted with it at that time, and also that others might be induced in this way to publish the results of their speculations and might not complain that the glory of the discovery had been snatched from them. Afterwards, however, in response to the request of the same distinguished man, I also solved this literary riddle, and set before him in outline the entire hypothesis; whence perhaps my theory about the phases of Saturn has already found its way to the ears of others. But, in any case, the wonderful and unusual creation of nature shown in connection with this planet demands a fuller treatment, and I ought not to expect that either my account of the phenomena or the assumptions I make for explaining them will gain general support unless it is seen that the latter rest on the principles of reasoning, and the former is backed by the evidence of observation. Therefore, I now propose to fulfil both of these requirements. And, in the first place, I will determine as accurately as possible from my observations the facts which have to do with the motion and the period of revolution of the planet's satellite, and I will construct tables of its motion. Then I will assign the various phases of Saturn himself to their separate causes, that thus we may have a ready means of determining beforehand what the future phases will be . . .

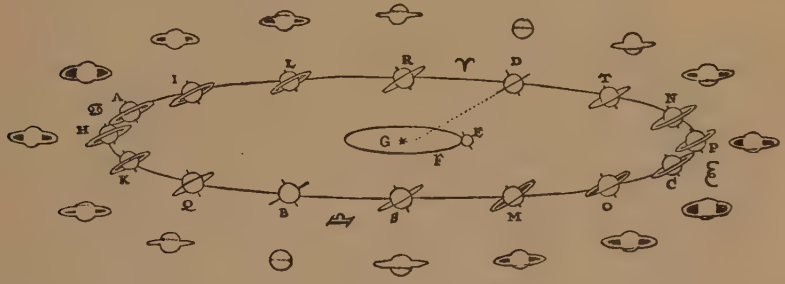
That neither you [Hodierna] nor those distinguished men whose opinions I have previously reviewed have reached the truth of the matter, is not at all to be wondered at or to be imputed to you as a fault, since for the most part false phenomena were reported to you as true, and other phenomena which were observed in connection with Saturn, free from the deception of sight, did not come to your notice at all. If you had been so fortunate as to observe these phenomena with me, it is reasonable to suppose that you would have drawn from them the same conclusions with regard to the real form of the planet that I have. Now I was greatly helped in this matter not only by those more genuine phases, but also by the motion of Saturn's Moon, which I observed from the beginning; indeed it was the revolution of this Moon around Saturn that first caused to dawn upon me the hope of constructing the hypothesis. The nature of this hypothesis I will proceed to explain in what follows.

When, then, I had discovered that the new planet revolved around Saturn in a period of sixteen days, I thought that without

any doubt Saturn rotated on his own axis in even less time. For even before this I had always believed that the other primary planets were like our Earth in this respect that each rotated on its own axis, and so the entire surface rejoiced in the light of the Sun, a part at a time; and, more than this, I believe that in general the arrangement with the large bodies of the world was such that those around which smaller bodies revolved, having themselves a central position, had also a shorter period of rotation. Thus the Sun, its spots declare, rotates on its own axis in about twenty-six days; but around the Sun the various planets, among which the Earth is also to be reckoned, complete their courses in times varying as their distances. Again, this Earth rotates in daily course, and around the Earth the Moon circles with monthly motion. Around the planet Jupiter four smaller planets, that is to say Moons, revolve, subject to this same law, under which the velocities increase as the distances diminish. Whence, indeed, we must conclude perhaps that Jupiter rotates in a shorter time than 24 hours, since his nearest Moon requires less than two days. Now having long since learned all these facts, I concluded even then that Saturn must have a similar motion. But it was my observation in regard to his satellite that gave me the information about the velocity of his rotative motion. The fact that the satellite completes its orbit in sixteen days leads to the conclusion that Saturn, being in the center of the satellite's orbit, rotates in much less time. Furthermore, the following conclusion seemed reasonable: that all the celestial matter that lies between Saturn and his satellite is subject to the same motion, in this way that the nearer it is to Saturn, the nearer it approaches Saturn's velocity. Whence, finally, the following resulted: the appendages also, or arms, of Saturn are either joined and attached to the globular body at its middle and go around with it, or, if they are separated by a certain distance, still revolve at a rate not much inferior to that of Saturn.

Furthermore, while I was considering these facts in connection with the motion of the arms, these arms appeared under the aspect which was exhibited at the time of my previous observations of the year 1655. The body of Saturn at its middle was quite round, while the arms extended on either side along the same straight line, as though the planet were pierced through the middle by a kind of axis; although, as indicated in the first figure of all, these arms, as seen through the twelve-foot telescope that I was then using,

appeared a little thicker and brighter toward the ends on either side of the planet than they did where they joined the middle of the sphere. When, therefore, the planet continued day after day to present this same aspect, I came to understand that, inasmuch as the circuit of Saturn and the adhering bodies was so short, this could happen under no other condition than that the globe of Saturn were assumed to be surrounded equally on all sides by another body, and that thus a kind of ring encircled it about the middle; for so, with whatever velocity it revolved, it would always present the same aspect to us, if, of course, its axis were perpendicular to the plane of the ring.



The phases of Saturn's Ring. From Huygen's *Systema Saturnium.*

And so was established the reason for the phase which continued through that period. Therefore, after that, I began to consider whether the other phases that Saturn was said to have could be accounted for by the same ring. I was not long in coming to a conclusion on this point through noting in frequent observations the obliquity of Saturn's arms to the ecliptic. For when I had discovered that the straight line along which on either side these arms projected did not follow the line of the ecliptic, but cut it at an angle of more than 20 degrees, I concluded that in the same way the plane of the ring which I had imagined was inclined at about the same angle to the plane of the ecliptic—with a permanent and unchanging inclination, be it understood, as is known to be the case on this Earth of ours with the plane of the equator. From this inclination it necessarily followed that in its different aspects the same ring showed to us at one time a rather broad ellipse, at another time a narrower ellipse, and sometimes even a straight line. As regards the handle-like formations, I understand that this phenomenon was due to the fact that the ring was not attached to

the globe of Saturn, but was separated from it the same distance all around. These facts, accordingly, being thus brought into line, and the above-mentioned inclination of the ring being also assumed, all the wonderful appearance of Saturn, I found, could be referred to this source, as will presently be shown. And this is that very hypothesis which, in the year 1656, on the 25th day of March,[1] I put forth in confused letters, together with my observation on the Saturnian Moon.

Now the letters were: a a a a a a a a c c c c c d e e e e e g h i i i i i i i i l l l l m m n n n n n n n n n n n o o o o o p p q r r s t t t t t t u u u u u u, which, being restored to their proper places, signify the following: *Annulo cingitur, tenui, plano, nusquam cohærente, ad eclipticam inclinato.*[2] That the width of the space intervening between the ring and the globe of Saturn is equal to the width of the ring itself or even exceeds it, is shown by the figure of Saturn as observed by others, and then more definitely by its figure as seen by myself; that, likewise, the ratio of the greatest diameter of the ring to the diameter of Saturn is about 9 to 4. Thus the true appearance is such as I have indicated in the appended scheme.

I believe that I should digress here to meet the objection of those who will find it exceedingly strange and possibly unreasonable that I should assign to one of the celestial bodies a figure the like of which has up to this time not been found in any one of them, although, on the other hand, it has been believed as certain, and considered as established by natural law, that the spherical form is the only one adapted to them; and that I should place this solid and permanent ring (for such I consider it) about Saturn, without attaching it by any joints or ties, although imagining that it preserves a uniform distance on every side and revolves in company with Saturn at a very high rate of speed. These men should consider that I do not construct this hypothesis from pure invention and out of my own fancy, as the astronomers do their epicycles, which nowhere appear in the heavens, but that I perceive this ring very plainly with the eyes; with which, obviously, we discern the figures of all other things. And there is, after all, no reason why it should not be possible for some heavenly body to exist having this form, which, if not spherical, is at least round, and is quite as well adapted to the possession of circumcentral motion as the spherical

[1 Earlier Huygens says the fifth of March (see p. 64).]

[2 "It is encircled by a ring, thin, plane, nowhere attached, inclined to the ecliptic."]

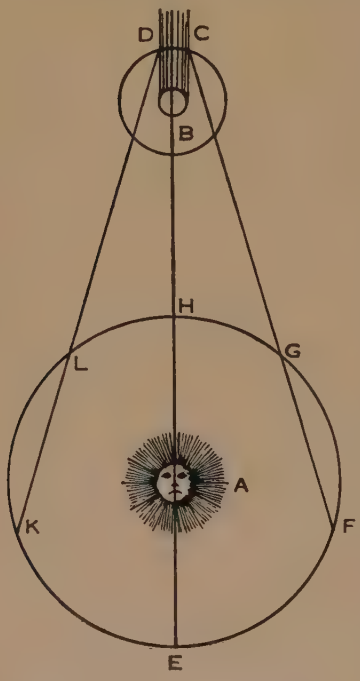
form itself. For it certainly is less surprising that such a body should have assigned to it a shape of this kind than that it should have some absurd and quite unbeautiful shape. Furthermore, since, owing to the great similarity and relationship that exists between Saturn and our Earth, it seems possible to conclude quite conclusively that the former, like the latter, is situated in the middle of its own vortex, and that its center has a natural tendency to reach toward all that is considered to have weight there, it must also result that the ring in question, pressing with all its parts and with equal force toward the center, comes by this very fact to a permanent position in such a way that it is equally distant on all sides from that center. Exactly so some people have imagined that, if it were possible to construct a continuous arch all the way around the Earth, it would sustain itself without any support. Therefore, let them not consider it absurd if a similar thing has happened of itself in the case of Saturn; let them rather regard with awe the power and majesty of Nature, which, by repeatedly bringing to light new specimens of its works, admonishes us that yet more remain.

ROEMER[1]

The Finite Velocity of Light

(Translated from "Mémoires de l'Académie Royale des Sciences," Tome X, 1730.)

For a long time philosophers have been anxious to determine, by some experiment, whether the action of light is carried any distance instantaneously, or whether it requires time. Roemer of the Royal Academy of Sciences has found a method, derived from observations of the first satellite of Jupiter, whereby he shows that light does not take one second of time to traverse a distance of about three thousand leagues, nearly the size of the diameter of the Earth.



Let *A* be the Sun, *B* Jupiter, *C* the first satellite which enters the shadow of Jupiter, to come out at *D*, and let *E F G H K L* be the Earth, placed at various distances from Jupiter.

Now supposing that the Earth when at *L*, near the second quadrature of Jupiter, has seen the first satellite at the time of its emersion or coming out of the shadow at *D*; and supposing that about 42½ hours afterwards, *i.e.*, after a revolution of the satellite, the Earth being at *K*, the return path at *D*, it is evident that if light takes time to cross the intervening space *LK*, the satellite will be

[1] Olaus Roemer (1644–1710), Danish astronomer, working at the Paris Observatory, measured the velocity of light by a study of the motions and eclipses of Jupiter's satellites. He constructed an early transit instrument, as well as pioneer examples of equatorial and altazimuth telescopes.

seen at *D* later than it would have been seen if the Earth had remained at *L*, so that the revolution of the satellite, thus observed by means of its emersions, will be retarded by as much time as the light will have taken to pass from *L* to *K*. On the other hand, in the other quadrature *FG*, the Earth when approaching goes before the light, and the succession of the immersions will appear shortened by as much as those of the emersions had appeared lengthened. Since in the 42½ hours that the satellite takes approximately to make each revolution, the distance between the Earth and Jupiter, in both quadratures, varies at least 210 diameters of the Earth, it follows that if a second of time is necessary for each diameter of the Earth, the light would require 3½ minutes for each of these intervals *FG*, *KL*, which would cause a difference of about an eighth of an hour between two revolutions of the first satellite, when one is observed at *FG*, and the other at *KL*, whereas no perceptible difference is observed.

It does not follow, however, that light requires no time at all; for after examining the matter more closely, it was found that what was not obvious in two revolutions became very considerable in regard to several taken together, and that, for example, 40 revolutions observed on the side *F* were sensibly shorter than 40 others observed on the other side, and this amounted to 22 minutes for the entire distance *HE*, which is double that from here to the Sun. [The present value of this constant is 16 minutes, 37 seconds.]

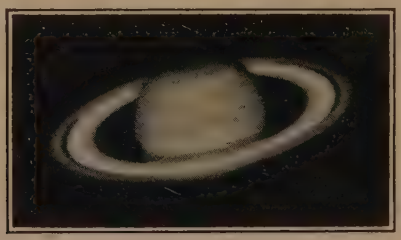
The necessity of this new equation for the retardation of light is shown by all the observations which have been made during eight years at the Royal Academy and the Observatory, and it has been newly confirmed by the emersion of the first satellite, observed at Paris on the ninth of last November at 5 hours, 35 minutes, 45 seconds in the evening, ten minutes later than one should have expected in deducing the emersions from those which had been observed during the month of August when the Earth was much nearer Jupiter—an event which Roemer had been predicting to the Academy since the beginning of September; but in order to remove every reason of doubting that this inequality is caused by the retardation of light, he shows that it cannot come from any eccentricity, or any of those causes which one ordinarily proposes to explain the irregularities of the Moon and of the other planets.

CASSINI[1]

The Discovery of the Division in Saturn's Ring

(Translated from "Mémoires de l'Académie Royale des Sciences," Tome X, 1730.)

After the discoveries which have been made at different times concerning the globe of Saturn, its ring and its satellites, in part by Huyghens who discovered one of the satellites which revolves around Saturn in 16 days less 47 minutes, and in part by Cassini who discovered two others of which we will give the history at an early date, it seemed that there was nothing more to discover concerning the planet; however, the latest observations that



Photograph of Saturn, showing Cassini's division in the ring. *Mount Wilson Observatory.*

Cassini has made concerning the body of Saturn and its ring, show that in the Heavens as well as on the Earth, something new to observe always appears.

After the emergence of Saturn from the rays of the Sun as a morning star in the year 1675, the globe of the planet appeared with a dark band, similar to those of Jupiter, extending the length of the ring from East to West, as it is nearly always shown by the 34-foot telescope, and the breadth of the ring was divided by a dark line into two equal parts, of which the interior and nearer one to the

[1] Giovanni Domenico Cassini (1625–1712), Italian astronomer, was appointed the first director of the Paris Observatory. He is especially known for his planetary work: the discovery of the division in Saturn's ring and four satellites of Saturn, the tables of Jupiter's moons, and work on the rotation of planets.

globe was very bright, and the exterior part slightly dark. There was about the same difference between the colors of these two parts that there is between dull silver and burnished silver, which had never before been observed but which has since been seen in the same telescope, more clearly at twilight and in moonlight than on a darker night.

This appearance gave an impression of a double ring, of which the *inferior* ring, being larger and darker, had superposed upon it another that is narrower and brighter, and reminds one that in the year 1671, when the extensions of Saturn were on the verge of disappearing they contracted beforehand, perhaps because the outer part of the ring, which was single and dark, disappeared before the inner part, which was double and brighter.

In the same year, 1671, the shorter diameter of the ring was still less than the diameter of the globe which extended outside the ring on the North and South sides, and this phase lasted until the immersion of Saturn in the rays of the Sun in the year 1676. But after its emersion, which took place last summer, the shorter diameter of the ring exceeded that of the globe. There is an observation by Hevelius in the English Journal, which corresponds to the first of these two phases; but as he has noted neither the band of Saturn, nor the distinction which can be seen in the ring, one has reason to judge that the telescopes which he uses are much inferior to those of the Royal Observatory.

NEWTON[1]

Principles of Natural Philosophy

(From Newton's "Principia," third edition, 1726; translated by Andrew Motte, 1729, first American edition, 1848.)

Axioms, or Laws of Motion.—Law I. Every body perseveres in its state of rest, or of uniform motion in a right line, unless it is compelled to change that state by force impressed thereon.

Projectiles persevere in their motions, so far as they are not retarded by the resistance of the air, or impelled downwards by the force of gravity. A top, whose parts by their cohesion are perpetually drawn aside from rectilinear motions, does not cease its rotation, otherwise than as it is retarded by the air. The greater bodies of the planets and comets, meeting with less resistance in more free spaces, preserve their motions both progressive and circular for a much longer time.

Law II. The alteration of motion is ever proportional to the motive force impressed; and is made in the direction of the right line in which that force is impressed.

If any force generates a motion, a double force will generate double the motion, a triple force triple the motion, whether that force be impressed altogether and at once, or gradually and successively. And this motion (being always directed the same way with the generating force), if the body moved before, is added to or subducted from the former motion, according as they directly conspire with or are directly contrary to each other; or obliquely joined, when they are oblique, so as to produce a new motion compounded from the determination of both.

Law III. To every action there is always opposed an equal reaction: or the mutual actions of two bodies upon each other are always equal, and directed to contrary parts.

[1] Isaac Newton (1642–1727), the greatest of English astronomers—a mathematician and natural philosopher. Newton's studies of the physical laws of the universe stand as probably the greatest achievement of the human intellect. "Taking mathematics from the beginning of the world to the time when Newton lived, what he had done was much the better half" (Leibniz).

Whatever draws or presses another is as much drawn or pressed by that other. If you press a stone with your finger, the finger is also pressed by the stone. If a horse draws a stone tied to a rope, the horse (if I may so say) will be equally drawn back towards the stone: for the distended rope, by the same endeavour to relax or unbend itself, will draw the horse as much towards the stone, as it does the stone towards the horse, and will obstruct the progress of the one as much as it advances that of the other. If a body impinge upon another, and by its force change the motion of the other, that body also (because of the equality of the mutual pressure) will undergo an equal change, in its own motion, towards the contrary part. The changes made by these actions are equal, not in the velocities but in the motions of bodies; that is to say, if the bodies are not hindered by any other impediments. For, because the motions are equally changed, the changes of the velocities made towards contrary parts are reciprocally proportional to the bodies. This law takes place also in attractions, as will be proved in the next scholium . . .

Rules of Reasoning in Philosophy.—Rule I. We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances.

To this purpose the philosophers say that Nature does nothing in vain, and more is in vain when less will serve; for Nature is pleased with simplicity, and affects not the pomp of superfluous causes.

Rule II. Therefore to the same natural effects we must, as far as possible, assign the same causes.

As to respiration in a man and in a beast; the descent of stones in *Europe* and in *America;* the light of our culinary fire and of the sun; the reflection of light in the earth, and in the planets.

Rule III. The qualities of bodies, which admit neither intension nor remission of degrees, and which are found to belong to all bodies within the reach of our experiments, are to be esteemed the universal qualities of all bodies whatsoever.

For since the qualities of bodies are only known to us by experiments, we are to hold for universal all such as universally agree with experiments; and such as are not liable to diminution can never be quite taken away. We are certainly not to relinquish the evidence of experiments for the sake of dreams and vain fictions of our own devising; nor are we to recede from the analogy

of Nature, which uses to be simple, and always consonant to itself. We no other way know the extension of bodies than by our senses, nor do these reach it in all bodies; but because we perceive extension in all that are sensible, therefore, we ascribe it universally to all others also. That abundance of bodies are hard, we learn by experience; and because the hardness of the whole arises from the hardness of the parts, we, therefore, justly infer the hardness of the undivided particles not only of the bodies we feel but of all others. That all bodies are impenetrable, we gather not from reason, but from sensation. The bodies which we handle we find impenetrable, and thence, conclude impenetrability to be an universal property of all bodies whatsoever. That all bodies are moveable, and endowed with certain powers (which we call the *vires inertiæ*) of persevering in their motion, or in their rest, we only infer from the like properties observed in the bodies which we have seen. The extension, hardness, impenetrability, mobility, and *vis inertiæ* of the whole, result from the extension, hardness, impenetrability, mobility, and *vires inertiæ* of the parts; and thence we conclude the least particles of all bodies to be also all extended, and hard and impenetrable, and moveable, and endowed with their proper *vires inertia*. And this is the foundation of all philosophy. Moreover, that the divided but contiguous particles of bodies may be separated from one another is matter of observation; and, in the particles that remain undivided, our minds are able to distinguish yet lesser parts, as is mathematically demonstrated. But whether the parts so distinguished, and not yet divided, may, by the powers of Nature, be actually divided and separated from one another, we cannot certainly determine. Yet, had we the proof of but one experiment that any undivided particle, in breaking a hard and solid body, suffered a division, we might by virtue of this rule, conclude that the undivided as well as the divided particles may be divided and actually separated to infinity.

Lastly, if it universally appears, by experiments and astronomical observations, that all bodies about the earth gravitate towards the earth, and that in proportion to the quantity of matter which they severally contain; that the moon likewise, according to the quantity of its matter, gravitates towards the earth; that, on the other hand, our sea gravitates towards the moon; and all the planets mutually one towards another; and the comets in like manner towards the sun; we must, in consequence of this rule, universally allow that all bodies whatsoever are endowed with a

principle of mutual gravitation. For the argument from the appearances concludes with more force for the universal gravitation of all bodies than for their impenetrability; of which, among those in the celestial regions, we have no experiments, nor any manner of observation. Not that I affirm gravity to be essential to bodies: by their *vis insita* I mean nothing but their *vis inertiæ*. This is immutable. Their gravity is diminished as they recede from the earth.

Rule IV. In experimental philosophy we are to look upon propositions collected by general induction from phænomena as accurately or very nearly true, notwithstanding any contrary hypotheses that may be imagined, till such time as other phænomena occur, by which they may either be made more accurate, or liable to exceptions.

This rule we must follow, that the argument of induction may not be evaded by hypotheses.

Concerning the Law of Gravitation.—Hitherto we have explained the phænomena of the heavens and of our sea by the power of gravity, but have not yet assigned the cause of this power. This is certain, that it must proceed from a cause that penetrates to the very centres of the sun and planets, without suffering the least diminution of its force; that operates not according to the quantity of the surfaces of the particles upon which it acts (as mechanical causes use to do), but according to the quantity of the solid matter which they contain, and propagates its virtue on all sides to immense distances, decreasing always in the duplicate proportion of the distances. Gravitation towards the sun is made up out of the gravitations towards the several particles of which the body of the sun is composed; and in receding from the sun decreases accurately in the duplicate proportion of the distances as far as the orb of Saturn, as evidently appears from the quiescence of the aphelions of the planets; nay, and even to the remotest aphelions of the comets, if those aphelions are also quiescent. But hitherto I have not been able to discover the cause of those properties of gravity from phænomena, and I frame no hypotheses; for whatever is not deduced from the phænomena is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy. In this philosophy particular propositions are inferred from the phænomena, and afterwards rendered general by induction. Thus it was that the impenetrability, the mobility,

and the impulsive force of bodies, and the laws of motion and of gravitation, were discovered. And to us it is enough that gravity does really exist, and act according to the laws which we have explained, and abundantly serves to account for all the motions of the celestial bodies, and of our sea.

Illustration from Hevelius' *Prodromus Astronomiæ cum Catalogo Fixarum*, 1690.

And now we might add something concerning a certain most subtle Spirit which pervades and lies hid in all gross bodies; by the force and action of which Spirit the particles of bodies mutually attract one another at near distances, and cohere, if contiguous; and electric bodies operate to greater distances, as well repelling

as attracting the neighbouring corpuscles; and light is emitted, reflected, refracted, inflected, and heats bodies; and all sensation is excited, and the members of animal bodies move at the command of the will, namely, by the vibrations of this Spirit, mutually propagated along the solid filaments of the nerves, from the outward organs of sense to the brain, and from the brain into the muscles. But these are things that cannot be explained in few words, nor are we furnished with that sufficiency of experiments which is required to an accurate determination and demonstration of the laws by which this electric and elastic Spirit operates.

The System of the World

Concerning the Orbits in the Planetary System.—Because the fixed stars are quiescent, one in respect of another, we may consider the sun, earth, and planets, as one system of bodies carried hither and thither by various motions among themselves; and the common centre of gravity of all[1] will either be quiescent, or move uniformly forward in a right line: in which case, the whole system will likewise move uniformly forward in right lines. But this is an hypothesis hardly to be admitted; and, therefore, setting it aside, that common centre will be quiescent: and from it the sun is never far removed. The common centre of gravity of the sun and Jupiter falls on the surface of the sun; and though all the planets were placed towards the same parts from the sun with Jupiter the common centre of the sun and all of them would scarcely recede twice as far from the sun's centre; and, therefore, though the sun, according to the various situations of the planets, is variously agitated, and always wandering to and fro with a slow motion of libration, yet it never recedes one entire diameter of its own body from the quiescent centre of the whole system. But from the weights of the sun and planets above determined, and the situation of all among themselves, their common centre of gravity may be found; and, this being given, the sun's place to any supposed time may be obtained.

About the sun thus librated the other planets are revolved in elliptic orbits, and, by radii drawn to the sun, describe areas nearly proportional to the times.[2] If the sun was quiescent, and

[1] Cor. IV of the Laws of Motion.

[2] As is explained in Prop. LXV.

the other planets did not act mutually, one upon another, their orbits would be elliptic, and the areas exactly proportional to the times.[1] But the actions of the planets among themselves, compared with the actions of the sun on the planets, are of no moment, and produce no sensible errors. And those errors are less in revolutions about the sun agitated in the manner but now described than if those revolutions were made about the sun quiescent,[2] especially if the focus of every orbit is placed in the common centre of gravity of all the lower included planets; *viz.*, the focus of the orbit of Mercury in the centre of the Sun; the focus of the orbit of Venus in the common centre of gravity of Mercury and the sun; the focus of the orbit of the earth in the common centre of gravity of Venus, Mercury, and the sun; and so of the rest. And by this means the foci of the orbits of all the planets, except Saturn, will not be sensibly removed from the centre of the sun, nor will the focus of the orbit of Saturn recede sensibly from the common centre of gravity of Jupiter and the sun. And, therefore, astronomers are not far from the truth, when they reckon the sun's centre the common focus of all the planetary orbits. In Saturn itself the error thence arising does not exceed 1′45″. And if its orbit, by placing the focus thereof in the common centre of gravity of Jupiter and the sun, shall happen to agree better with the phænomena, from thence all that we have said will be farther confirmed.

If the sun was quiescent, and the planets did not act one on another, the aphelions and nodes of their orbits would likewise[3] be quiescent. And the longer axes of their elliptic orbits would[4] be as the cubic roots of the squares of their periodic times: and, therefore, from the given periodic times would be also given. But those times are to be measured not from the equinoctial points, which are moveable, but from the first star of Aries. Put the semi-axis of the earth's orbit 100,000, and the semi-axes of the orbits of Saturn, Jupiter, Mars, Venus, and Mercury, from their periodic times, will come out 953,806, 520,116, 152,399, 72,333, 38,710, respectively. But from the sun's motion every semi-axis is increased[5] by about one third of the distance of the sun's centre

[1] By Prop. XI, and Cor. 1, Prop. XIII.
[2] By Prop. LXVI, and Cor. Prop. LXVIII.
[3] By Prop. 1, XI, and Cor. Prop. XIII.
[4] By Prop. XV.
[5] By Prop. LX.

from the common centre of gravity of the sun and planet. And from the actions of the exterior planets on the interior, the periodic times of the interior are something protracted, though scarcely by any sensible quantity; and their aphelions are transferred[1] by very slow motions in *consequentia*. And on the like account the periodic times of all, especially of the exterior planets, will be prolonged by the actions of the comets, if any such there are, without the orb of Saturn, and the aphelions of all will be thereby carried forwards in *consequentia*. But from the progress of the aphelions the regress of the nodes follows.[2] And if the plane of the ecliptic is quiescent, the regress of the nodes[3] will be to the progress of the aphelion in every orbit as the regress of the nodes of the moon's orbit to the progress of its apogeon nearly, that is, as about 10 to 21. But astronomical observations seem to confirm a very slow progress of the aphelions, and a regress of the nodes in respect of the fixed stars. And hence it is probable that there are comets in the regions beyond the planets which, revolving in very eccentric orbs, quickly fly through their perihelion parts, and by an exceedingly slow motion in their aphelions, spend almost their whole time in the regions beyond the planets; as we shall afterwards explain more at large.

The planets thus revolved about the sun may at the same time carry others revolving about themselves as satellites or moons.[4] But from the action of the sun our moon must move with greater velocity and, by a radius drawn to the earth, describe an area greater for the time; it must have its orbit less curved and, therefore, approach nearer to the earth in the syzygies than in the quadratures, except in so far as the motion of eccentricity hinders those effects. For the eccentricity is greatest when the moon's apogeon is in the syzygies, and least when the same is in the quadratures; and hence it is that the perigeon moon is swifter and nearer to us but the apogeon moon slower and farther from us, in the syzygies than in the quadratures. But farther; the apogeon has a progressive and the nodes a regressive motion, both unequable. For the apogeon is more swiftly progressive in its syzygies, more slowly regressive in its quadratures and by the excess of its progress above its regress is yearly transferred in *consequentia;*

[1] By Cor. VI and VII, Prop. LXVI.
[2] By Cor. XI, XIII, Prop. LXVI.
[3] By Cor. XVI, Prop. LXVI.
[4] As appears by Prop. LXVI.

but the nodes are quiescent in their syzygies, and most swiftly regressive in their quadratures. But farther, still, the greatest latitude of the moon is greater in its quadratures than in its syzygies; and the mean motion swifter in the aphelion of the earth than in its perihelion. Most inequalities in the moon's motion have not hitherto been taken notice of by astronomers: but all these follow from our principles,[1] and are known really to exist in the heavens. And this may be seen in that most ingenious, and if I mistake not, of all the most accurate hypothesis of Mr. *Horrox*, which Mr. *Flamsted* has fitted to the heavens; but the astronomical hypotheses are to be corrected in the motion of the nodes; for the nodes admit the greatest equation or prosthaphæresis in their octants, and this inequality is most conspicuous when the moon is in the nodes, and, therefore, also in the octants; and hence it was that *Tycho*, and others after him, referred this inequality to the octants of the moon, and made it menstrual; but the reasons by us adduced prove that it ought to be referred to the octants of the nodes, and to be made annual.

Beside those inequalities, taken notice of by astronomers, there are yet some others, by which the moon's motions are so disturbed, that hitherto by no law could they be reduced to any certain regulation. For the velocities or horary motions of the apogee and nodes of the moon and their equations, as well as the difference betwixt the greatest eccentricity in the syzygies and the least in the quadratures and that inequality which we call the variation, in the progress of the year are augmented and diminished[2] in the triplicate ratio of the sun's apparent diameter. Beside that, the variation is mutable nearly in the duplicate ratio of the time between the quadratures;[3] and all those inequalities are something greater in that part of the orbit which respects the sun than in the opposite part, but by a difference that is scarcely or not at all perceptible . . .

Concerning the Moon's Libration and Rotational Flattening.— While the planets are thus revolved in orbits about remote centers, in the mean time they make their several rotations about their proper axes; the sun in 26 days; Jupiter in 9^h 56′; Mars in $24\frac{2}{3}{}^h$; Venus in 23^h; and that in planes not much inclined to the plane of

[1] In Cor. II, III, IV, V, VI, VII, VIII, IX, X, XI, XII, XIII, Prop. LXVI.
[2] By Cor. XIV, Prop. LXVI.
[3] By Cor. I and II, Lem. X, and Cor. XVI, Prop. LXVI.

the ecliptic, and according to the order of the signs, as astronomers determine from the spots or maculæ that by turns present themselves to our sight in their bodies; and there is a like revolution of our earth performed in 24^h; and those motions are neither accelerated nor retarded by the actions of the centripetal forces;[1] and, therefore, of all others they are the most equable and most fit for the mensuration of time; but those revolutions are to be reckoned equable not from their return to the sun but to some fixed star: for as the position of the planets to the sun is unequably varied, the revolutions of those planets from sun to sun are rendered unequable.

In like manner is the moon revolved about its axis by a motion most equable in respect of the fixed stars, *viz.*, in $27^d\ 7^h\ 43'$, that is, in the space of a sidereal month; so that this diurnal motion is equal to the mean motion of the moon in its orbit; upon which account, the same face of the moon always respects the center about which this mean motion is performed, that is, the exterior focus of the moon's orbit nearly; and hence arises a deflection of the moon's face from the earth, sometimes towards the east and other times toward the west, according to the position of the focus which it respects; and this deflection is equal to the equation of the moon's orbit, or to the difference betwixt its mean and true motions; and this is the moon's libration in longitude: but it is likewise affected with a libration in latitude arising from the inclination of the moon's axis to the plane of the orbit in which the moon is revolved about the earth; for that axis retains the same position to the fixed stars nearly, and hence the poles present themselves to our view by turns, as we may understand from the example of the motion of the earth whose poles, by reason of the inclination of its axis to the plane of the ecliptic, are by turns illuminated by the sun. To determine exactly the position of the moon's axis to the fixed stars, and the variation of this position, is a problem worthy of an astronomer.

By reason of the diurnal revolutions of the planets, the matter which they contain endeavours to recede from the axis of this motion; and hence the fluid parts rising higher towards the equator than about the poles, would lay the solid parts about the equator under water, if those parts did not rise also: upon which account the planets are something thicker about the equator than about the poles; and their equinoctial points thence become regressive; and their axes, by a motion of nutation, twice in every revolution,

[1] As appears by Cor. XXII, Prop. LXVI.

librate towards their ecliptics, and twice return again to their former inclination;[1] and hence it is that Jupiter, viewed through very long telescopes, does not appear altogether round, but having its diameter that lies parallel to the ecliptic something longer than that which is drawn from north to south.

Concerning the Tides.—And from the diurnal motion and the attractions of the sun and moon our sea ought twice to rise and twice to fall every day, as well lunar as solar,[2] and the greatest height of the water to happen before the sixth hour of either day and after the twelfth hour preceding. By the slowness of the diurnal motion the flood is retracted to the twelfth hour; and by the force of the motion of reciprocation it is protracted and deferred till a time nearer to the sixth hour. But till that time is more certainly determined by the phenomena, choosing the middle between those extremes, why may we not conjecture the greatest height of the water to happen at the third hour? For thus the water will rise all that time in which the force of the luminaries to raise it is greater, and will fall all that time in which their force is less; *viz.*, from the ninth to the third hour, when that force is greater, and from the third to the ninth when it is less. The hours I reckon from the appulse of each luminary to the meridian of the place as well under as above the horizon; and by the hours of the lunar day I understand the twenty-fourth parts of that time which the moon spends before it comes about again by its apparent diurnal motion to the meridian of the place which it left the day before.

But the two motions which the two luminaries raise will not appear distinguished, but will make a certain mixed motion. In the conjunction or opposition of the luminaries their forces will be conjoined, and bring on the greatest flood and ebb. In the quadratures the sun will raise the waters, which the moon depresseth and depress the waters which the moon raiseth; and from the difference of their forces the smallest of all tides will follow. And because (as experience tells us) the force of the moon is greater than that of the sun, the greatest height of the water will happen about the third lunar hour. Out of the syzygies and quadratures the greatest tide which by the single force of the moon ought to fall out

[1] As is explained in Cor. XVIII, Prop. LXVI.
[2] By Cor. XIX, XX, Prop. LXVI.

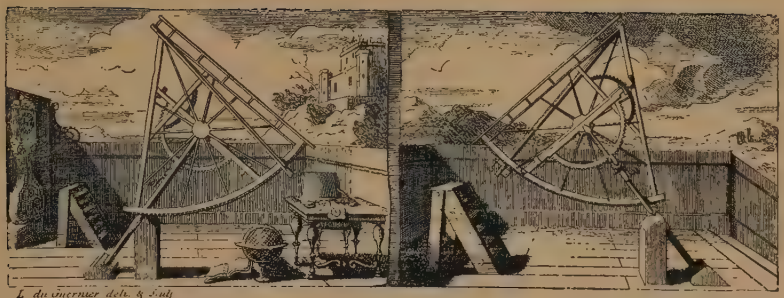
at the third lunar hour, and by the single force of the sun at the third solar hour, by the compounded forces of both must fall out in an intermediate time that approaches nearer to the third hour of the moon than to that of the sun; and, therefore, while the moon is passing from the syzygies to the quadratures, during which time the third hour of the sun precedes the third of the moon, the greatest tide will precede the third lunar hour, and that by the greatest interval a little after the octants of the moon; and by like intervals the greatest tide will follow the third lunar hour, while the moon is passing from the quadratures to the syzygies.

But the effects of the luminaries depend upon their distances from the earth; for when they are less distant their effects are greater, and when more distant their effects are less, and that in the triplicate proportion of their apparent diameters. Therefore, it is that the sun in the winter time, being then in its perigee, has a greater effect, and makes the tides in the syzygies something greater, and those in the quadratures something less, *cæteris paribus*, than in the summer season; and every month the moon, while in the perigee, raiseth greater tides than at the distance of 15 days before or after, when it is in its apogee. Whence it comes to pass that two highest tides do not follow one the other in two immediately succeeding syzygies.

The effect of either luminary doth likewise depend upon its declination or distance from the equator; for if the luminary was placed at the pole, it would constantly attract all the parts of the waters, without any intension or remission of its action, and could cause no reciprocation of motion; and, therefore, as the luminaries decline from the equator towards either pole, they will by degrees lose their force, and on this account will excite lesser tides in the solstitial than in the equinoctial syzygies. But in the solstitial quadratures they will raise greater tides than in the quadratures about the equinoxes; because the effect of the moon, then situated in the equator, most exceeds the effect of the sun; therefore, the greatest tides fall out in those syzygies, and the least in those quadratures, which happen about the time of both equinoxes; and the greatest tide in the syzygies is always succeeded by the least tide in the quadratures, as we find by experience. But because the sun is less distant from the earth in winter than in summer, it comes to pass that the greatest and least tides more frequently appear before than after the vernal equinox and more frequently after than before the autumnal . .

Thus we have seen that these forces are sufficient to move the sea. But, so far as I can observe, they will not be able to produce any other effect sensible on our earth; for since the weight of one grain in 4000 is not sensible in the nicest balance; and the sun's force to move the tides is 12,868,200 less than the force of gravity; and the sum of the forces of both moon and sun, exceeding the sun's force only in the ratio of $6\frac{1}{3}$ to 1, is still 2,032,890 times less than the force of gravity; it is evident that both forces together are 500 times less than what is required sensibly to increase or diminish the weight of any body in a balance. And, therefore, they will not sensibly move any suspended body; nor will they produce any sensible effect on pendulums, barometers, bodies swimming in stagnant water, or in the like statical experiments. In the atmosphere, indeed, they will excite such a flux and reflux as they do in the sea, but with so small a motion that no sensible wind will be thence produced.

Concerning the Distance of the Stars.—Thus I have given an account of the system of the planets. As to the fixed stars, the smallness of their annual parallax proves them to be removed to immense distances from the system of the planets: that this parallax is less than one minute is most certain; and from thence it follows that the distance of the fixed stars is above 360 times greater than the distance of Saturn from the sun. Such as reckon the earth one of the planets, and the sun one of the fixed stars, may remove the fixed stars to yet greater distances by the following arguments: from the annual motion of the earth there would happen an apparent transposition of the fixed stars, one in respect of another, almost equal to their double parallax; but the greater and nearer stars, in respect of the more remote, which are only seen by the telescope, have not hitherto been observed to have the least motion. If we should suppose that motion to be but less than 20″, the distance of the nearer fixed stars would exceed the mean distance of Saturn by above 2000 times. Again; the disk of Saturn, which is only 17″ or 18″ in diameter, receives but about $\frac{1}{2,100,000,000}$ of the sun's light; for so much less is that disk than the whole spherical surface of the orb of Saturn. Now if we suppose Saturn to reflect about one-fourth of this light, the whole light reflected from its illuminated hemisphere will be about $\frac{1}{4,200,000,000}$ of the whole light emitted from the sun's hemi-



L. du Guernier delt. & Sculp.

HISTORIÆ COELESTIS
LIBRI PRIMI,
PARS SECUNDA;
Stellarum Fixarum Distantias,
GRENOVICI
In Observatorio Regio,
Ab Anno 1676, ad Annum 1689,
Sextante Captas,
COMPLECTENS.

OBSERVATIONES FIXARUM.

Anno Christi MDCLXXVI. Mense, Die. Styl. Vet.	Temp. App. H. M.	In Constellatione Arietis.	Distantiæ per lineas diagonales. ° ′ ″	Distantiæ per Cochleæ Revolutiones & circuli partes Respondentes. Revol. Cent.	° ′ ″
☉ Oct. 29	12 02	*Aldebaran* & Arietis Prima —— γ		925 46	38 38 43
		Arietis Secunda —— β		918 24	38 20 39
		Arietis Lucida —— α		850 92	35 32 16
		Corn. & *Aust.* & —— ν		992 24	41 26 04
		—— γ		1253 14	52 19 55
☽ Nov. 6	10 50	*Aldebaran* & —— α		850 82	35 32 02
♀ 23	10 53	*Corn.* & *Bor.* & —— α		1070 95	44 43 18
	11 30	*Aldebaran* & —— γ		925 44	38 38 40
		—— β		918 20	38 20 33
		—— α		850 99	35 32 26

A

Illustration of the instruments and catalogue of John Flamsteed (1646–1719), the first Astronomer Royal. He adopted new methods in practical astronomy and constructed an accurate and extensive star catalogue, in which the average error is 10″; in Tycho's catalogue the average error is 60″.

sphere; and, therefore, since light is rarefied in the duplicate ratio of the distance from the luminous body, if the sun was 10,000 $\sqrt{42}$ times more distant than Saturn, it would yet appear as lucid as Saturn now does without its ring, that is, something more lucid than a fixed star of the first magnitude. Let us, therefore, suppose that the distance from which the sun would shine as a fixed star exceeds that of Saturn by about 100,000 times, and its apparent diameter[1] will be $7^{v}\ 16^{vi}$ and its parallax arising from the annual motion of the earth $13''''$: and so great will be the distance, the apparent diameter, and the parallax of the fixed stars of the first magnitude, in bulk and light equal to our sun. Some may, perhaps, imagine that a great part of the light of the fixed stars is intercepted and lost in its passage through so vast spaces and upon that account pretend to place the fixed stars at nearer distances; but at this rate the remoter stars could be scarcely seen. Suppose, for example, that three-fourths of the light perish in its passage from the nearest fixed stars to us; then three-fourths will twice perish in its passage through a double space, thrice through a triple, and so forth. And, therefore, the fixed stars that are at a double distance will be 16 times more obscure, *viz.*, 4 times more obscure on account of the diminished apparent diameter; and, again, 4 times more on account of the lost light. And, by the same argument, the fixed stars at a triple distance will be $9 \times 4 \times 4$, or 144 times more obscure; and those at a quadruple distance will be $16 \times 4 \times 4 \times 4$, or 1024 times more obscure; but so great a diminution of light is no ways consistent with the phænomena and with that hypothesis which places the fixed stars at different distances.

Concerning Comets.—The fixed stars being, therefore, at such vast distances from one another, can neither attract each other sensibly, nor be attracted by our sun. But the comets must unavoidably be acted on by the circumsolar force; for as the comets were placed by astronomers above the moon, because they were found to have no diurnal parallax, so their annual parallax is a convincing proof of their descending into the regions of the planets. For all the comets which move in a direct course, according to the order of the signs, about the end of their appearance become more than ordinarily slow, or retrograde, if the earth is between them and the sun; and more than ordinarily swift if

[1 The exponent ''' indicates a sixtieth of a second; the exponent '''' indicates a sixtieth of a sixtieth of a second, and so on.]

the earth is approaching to a heliocentric opposition with them. Whereas, on the other hand, those which move against the order of the signs, towards the end of their appearance, appear swifter than they ought to be if the earth is between them and the sun; and slower, and perhaps retrograde, if the earth is in the other side of its orbit. This is occasioned by the motion of the earth in different situations. If the earth go the same way with the comet, with a swifter motion, the comet becomes retrograde; if with a slower motion, the comet becomes slower, however; and if the earth move the contrary way, it becomes swifter; and by collecting the differences between the slower and swifter motions, and the sums of the more swift and retrograde motions, and comparing them with the situation and motion of the earth from whence they arise, I found, by means of this parallax, that the distances of the comets at the time they cease to be visible to the naked eye are always less than the distance of Saturn, and generally even less than the distance of Jupiter.

The same thing may be collected from the curvature of the way of the comets. These bodies go on nearly in great circles while their motion continues swift; but about the end of their course, when that part of their apparent motion which arises from the parallax bears a greater proportion to their whole apparent motion, they commonly deviate from those circles; and when the earth goes to one side, they deviate to the other; and this deflection, because of its corresponding with the motion of the earth, must arise chiefly from the parallax; and the quantity thereof is so considerable, as, by my computation, to place the disappearing comets a good deal lower than Jupiter. Whence it follows, that, when they approach nearer to us in their perigees and perihelions, they often descend below the orbits of Mars and the inferior planets.

Moreover, this nearness of the comets is confirmed by the annual parallax of the orbit, in so far as the same is pretty nearly collected by the supposition that the comets move uniformly in right lines. The method of collecting the distance of a comet according to this hypothesis from four observations (first attempted by *Kepler*, and perfected by Dr. *Wallis* and Sir *Christopher Wren*) is well known; and the comets reduced to this regularity generally pass through the middle of the planetary region. So the comets of the year 1607 and 1618, as their motions are defined by *Kepler*, passed between the sun and the earth; that of the year 1664 below the orbit of Mars; and that in 1680 below the orbit of Mercury, as its

motion was defined by Sir *Christopher Wren* and others. By a like rectilinear hypothesis, *Hevelius* placed all the comets about which we have any observations below the orbit of Jupiter. It is a false notion, therefore, and contrary to astronomical calculation, which some have entertained, who, from the regular motion of the comets, either remove them into the regions of the fixed stars, or deny the motion of the earth; whereas, their motions cannot be reduced to perfect regularity, unless we suppose them to pass through the regions near the earth in motion; and these are the arguments drawn from the parallax, so far as it can be determined without an exact knowledge of the orbits and motions of the comets.

The near approach of the comets is farther confirmed from the light of their heads; for the light of a celestial body, illuminated by the sun, and receding to remote parts, is diminished in the quadruplicate proportion of the distance; to wit, in one duplicate proportion on account of the increase of the distance from the sun; and in another duplicate proportion on account of the decrease of the apparent diameter. Hence it may be inferred, that Saturn being at a double distance, and having its apparent diameter nearly half of that of Jupiter, must appear about 16 times more obscure; and that, if its distance were 4 times greater, its light would be 256 times less; and, therefore, would be hardly perceivable to the naked eye. But now the comets often equal Saturn's light, without exceeding him in their apparent diameters. So the comet of the year 1668, according to Dr. *Hooke's* observations, equalled in brightness the light of a fixed star of the first magnitude; and its head, or the star in the middle of the coma, appeared, through a telescope of 15 feet, as lucid as Saturn near the horizon; but the diameter of the head was only 25″; that is, almost the same with the diameter of a circle equal to Saturn and his ring. The coma or hair surrounding the head was about ten times as broad; namely, 4⅙′. Again; the least diameter of the hair of the comet of the year 1682, observed by Mr. *Flamsted* with a tube of 16 feet and measured with the micrometer, was 2′0″; but the nucleus, or star in the middle, scarcely possessed the tenth part of this breadth, and was therefore only 11″ or 12″ broad; but the light and clearness of its head exceeded that of the year 1680, and was equal to that of the stars of the first or second magnitude. Moreover, the comet of the year 1665, in *April*, as *Hevelius* informs us, exceeded almost all the fixed stars in splendor, and even Saturn

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itself, as being of a much more vivid colour; for this comet was more lucid than that which appeared at the end of the foregoing year and was compared to the stars of the first magnitude. The diameter of the coma was about 6′; but the nucleus, compared with the planets by means of a telescope, was plainly less than Jupiter, and was sometimes thought less, sometimes equal to the body of Saturn within the ring. To this breadth add that of the ring, and the whole face of Saturn will be twice as great as that of the comet, with a light not at all more intense; and, therefore, the comet was nearer to the sun than Saturn. From the proportion of the nucleus to the whole head found by these observations, and from its breadth, which seldom exceeds 8′ or 12′, it appears that the stars of the comets are most commonly of the same apparent magnitude as the planets; but that their light may be compared oftentimes with that of Saturn, and sometimes exceeds it. And hence it is certain that in their perihelia their distances can scarcely be greater than that of Saturn. At twice that distance, the light would be four times less, which besides, by its dim paleness would be as much inferior to the light of Saturn as the light of Saturn is to the splendor of Jupiter; but this difference would be easily observed. At a distance ten times greater, their bodies must be greater than that of the sun; but their light would be 100 times fainter than that of Saturn. And at distances still greater, their bodies would far exceed the sun; but, being in such dark regions, they must be no longer visible. So impossible is it to place the comets in the middle regions between the sun and fixed stars, accounting the sun as one of the fixed stars; for certainly they would receive no more light there from the sun than we do from the greatest of the fixed stars.

So far we have gone without considering that obscuration which comets suffer from that plenty of thick smoke which encompasseth their heads, and through which the heads always shew dull as through a cloud; for by how much the more a body is obscured by this smoke, by so much the more near it must be allowed to come to the sun, that it may vie with the planets in the quantity of light which it reflects; whence it is probable that the comets descend far below the orbit of Saturn, as we proved before from their parallax. But, above all, the thing is evinced from their tails, which must be owing either to the sun's light reflected from a smoke arising from them, and dispersing itself through the æther, or to the light of their own heads . . .

Kepler ascribes the ascent of the tails of comets to the atmospheres of their heads, and their direction towards the parts opposite to the sun to the action of the rays of light carrying along with them the matter of the comets' tails; and without any great incongruity we may suppose that, in so free spaces, so fine a matter as that of the æther may yield to the action of the rays of the sun's light, though those rays are not able sensibly to move the gross substances in our parts, which are clogged with so palpable a resistance. Another author thinks that there may be a sort of particles of matter endowed with a principle of levity as well as others are with a power of gravity; that the matter of the tails of comets may be of the former sort, and that its ascent from the sun may be owing to its levity; but, considering the gravity of terrestrial bodies is as the matter of the bodies, and, therefore, can be neither more nor less in the same quantity of matter, I am inclined to believe that this ascent may rather proceed from the rarefaction of the matter of the comets' tails. The ascent of smoke in a chimney is owing to the impulse of the air with which it is entangled. The air rarefied by heat ascends, because its specific gravity is diminished, and in its ascent carries along with it the smoke with which it is engaged. And why may not the tail of a comet rise from the sun after the same manner? For the sun's rays do not act any way upon the mediums which they pervade but by reflection and refraction; and those reflecting particles heated by this action, heat the matter of the æther which is involved with them. That matter is rarefied by the heat which it acquires, and because by this rarefaction the specific gravity, with which it tended towards the sun before, is diminished, it will ascend therefrom like a stream, and carry along with it the reflecting particles of which the tail of the comet is composed; the impulse of the sun's light, as we have said, promoting the ascent.

But that the tails of comets do arise from their heads, and tend towards the parts opposite to the sun, is farther confirmed from the laws which the tails observe; for, lying in the planes of the comets' orbits which pass through the sun, they constantly deviate from the opposition of the sun towards the parts which the comets' heads in their progress along those orbits have left; and to a spectator placed in those planes they appear in the parts directly opposite to the sun; but as the spectator recedes from those planes, their deviation begins to appear, and daily becomes greater. And the deviation, *cæteris paribus*, appears less when the tail is more

oblique to the orbit of the comet, as well as when the head of the comet approaches nearer to the sun; especially if the angle of deviation is estimated near the head of the comet. Farther; the tails which have no deviation appear straight, but the tails which deviate are likewise bended into a certain curvature; and this curvature is greater when the deviation is greater, and is more sensible when the tail, *cæteris paribus*, is longer; for in the shorter tails the curvature is hardly to be perceived. And the angle of deviation is less near the comets' head, but greater towards the other end of the tail, and that because the lower side of the tail regards the parts from which the deviation is made, and which lie in a right line drawn out infinitely from the sun through the comet's head. And the tails that are longer and broader, and shine with a stronger light, appear more resplendent and more exactly defined on the convex than on the concave side. Upon which accounts it is plain that the phænomena of the tails of comets depend upon the motions of their heads, and by no means upon the places of the heavens in which their heads are seen; and that, therefore, the tails of the comets do not proceed from the refraction of the heavens, but from their own heads, which furnish the matter that forms the tail; for as in our air the smoke of a heated body ascends either perpendicularly, if the body is at rest, or obliquely if the body is moved obliquely, so in the heavens, where all the bodies gravitate towards the sun, smoke and vapour must (as we have already said) ascend from the sun, and either rise perpendicularly, if the smoking body is at rest, or obliquely, if the body, in the progress of its motion, is always leaving those places from which the upper or higher parts of the vapours had risen before. And that obliquity will be less where the vapour ascends with more velocity, to wit, near the smoking body, when that is near the sun; for there the force of the sun by which the vapour ascends is stronger. But because the obliquity is varied, the column of vapour will be incurvated; and because the vapour in the preceding side is something more recent, that is, has ascended something more lately from the body, it will therefore be something more dense on that side, and must on that account reflect more light, as well as be better defined; the vapour on the other side languishing by degrees, and vanishing out of sight.

HALLEY[1]

A Discussion of Elliptical Orbits of Comets

(From "A Synopsis of the Astronomy of Comets," in Gregory's "The Elements of Astronomy," Vol. 2, 1715.)

Hitherto I have consider'd the Orbits of Comets as exactly Parabolic; upon which supposition it wou'd follow, that Comets being impell'd towards the Sun by a Centripetal Force, would descend as from spaces infinitely distant, and by their so falling acquire such a Velocity, as that they may again fly off into the remotest parts of the Universe, moving upwards with a perpetual tendency, so as never to return again to the Sun. But since they appear frequently enough, and since some of them can be found to move with a Hyperbolic Motion, or a Motion swifter than what a Comet might acquire by its Gravity to the Sun, 'tis highly probable they rather move in very Excentric Elliptic Orbits, and make their returns after long periods of Time: For so their number will be determinate, and, perhaps, not so very great. Besides, the space between the Sun and the Fix'd Stars is so immense, that there is room enough for a Comet to revolve, tho' the Period of its revolution be vastly long. Now, the *Latus Rectum* of an Ellipsis, is to the *Latus Rectum* of a Parabola, which has the same Distance in its Perihelium; as the Distance in the Aphelium in the Ellipsis, is to the whole Axis of the Ellipsis. And the Velocities are in a Subduplicate ratio of the same: Wherefore in very Excentric Orbits the ratio comes very near to a ratio of Equality; and the very small difference which happens on account of the greater Velocity in the Parabola, is easily compensated in determining the situation of the Orbit. The principal use therefore, of this Table of the Elements of their Motions, and that which indeed induced me to construct it, is, that whenever a new Comet shall appear, we may be able to

[1] Edmond Halley (1656–1742), Astronomer Royal; his interest and persuasion encouraged Newton to enlarge and publish the Principia. Among Halley's important works were two predictions which could be verified only after his lifetime—the periodic reappearance of Halley's comet and the transits of Venus.

know, by comparing together the Elements, whether it be any of those which has appear'd before, and consequently to determine its Period, and the Axis of its Orbit, and to foretel its Return. And, indeed there are many things which make me believe that the Comet which *Apian* observ'd in the Year 1531, was the same with that which *Kepler* and *Longomontanus* more accurately describ'd in the Year 1607; and which I myself have seen return, and observ'd

Halley's Comet at the return in 1910. (Photographed at the *Harvard Observatory*.)

in the Year 1682. All the Elements agree, and nothing seems to contradict this my opinion, besides the Inequality of the Periodic revolutions. Which Inequality is not so great neither, as that it may not be owing to Physical Causes. For the Motion of Saturn is so disturbed by the rest of the Planets, especially Jupiter, that the Periodic time of that Planet is uncertain for some whole days together. How much more therefore will a Comet be subject to

such like errors, which rises almost four times higher than Saturn, and whose Velocity, tho' increased but a very little, would be sufficient to change its Orbit, from an Elliptical to a Parabolical one. And I am the more confirmed in my opinion of its being the same; for that in the Year 1456, in the Summer time, a Comet was seen passing Retrograde between the Earth and the Sun, much after the same manner: Which tho' nobody made observations upon it, yet from its Period and the manner of its Transit, I cannot think different from those I have just now mention'd. And since looking over the Histories of Comets I find, at an equal interval of Time, a Comet to have been seen about Easter in the Year 1305, which is another double Period of 151 Years before the former. Hence I think I may venture to foretel, that it will return again in the Year 1758. And, if it should then so return, we shall have no reason to doubt but the rest may return also: Therefore, Astronomers have a large field wherein to exercise themselves for many ages, before they will be able to know the number of these many and great Bodies revolving about the common Center of the Sun, and to reduce their Motions to certain Rules.

The Parallax of the Sun by the Transit of Venus

(From "Philosophical Transactions," *Abridged*, 1716.)

It is well known that this distance of the sun from the earth, is supposed different by different astronomers. Ptolemy and his followers, as also Copernicus and Tycho Brahe, have computed it at 1200 semi-diameters of the earth, and Kepler at almost 3500; Riccioli doubles this last distance, and Hevelius makes it only half as much. But at length it was found, on observing by the telescope, Venus and Mercury on the sun's disk, divested of their borrowed light, that the apparent diameters of the planets were much less than hitherto they had been supposed to be; and in particular, that Venus's semi-diameter, seen from the sun, only subtends the fourth part of a minute, or 15 seconds; and that Mercury's semi-diameter, at his mean distance from the sun, is seen under an angle of 10 seconds only, and Saturn's semi-diameter under the same angle; and that the semi-diameter of Jupiter, the largest of all the planets, subtends no more than the third part of a minute at the sun. Whence, by analogy, some modern astronomers conclude that the earth's semi-diameter, seen from the sun,

subtends a mean angle, between the greater of Jupiter and the less of Saturn and Mercury, and equal to that of Venus, *viz.* one of 15 seconds; and consequently, that the distance of the sun from the earth is almost 14,000 semi-diameters of the latter. Another consideration has made these authors enlarge this distance a little more: for since the moon's diameter is rather more than a quarter of the earth's diameter, if the sun's parallax be supposed 15 seconds, the body of the moon would be larger than that of Mercury, *viz.* a secondary planet larger than a primary one, which seems repugnant to the regular proportion and symmetry of the mundane system. On the contrary, it seems hardly consistent with the same proportion, that Venus, an inferior planet, and without any satellite, should be larger than our earth, a superior planet, and accompanied with so remarkable a satellite. Therefore, at a mean, supposing the earth's semi-diameter, seen from the sun, or which is the same thing, the sun's horizontal parallax, to be 12 seconds and a half, the moon will be less than Mercury, and the earth larger than Venus, and the sun's distance from the earth come out nearly 16,500 semi-diameters of the earth. I shall admit of this distance at present, till its precise quantity be made to appear more certain by the trial I propose; not regarding the authority of such as set the sun at an immensely greater distance, relying on the observations of a vibrating pendulum, which do not seem accurate enough to determine such minute angles; at least, such as use this method will find the parallax sometimes none at all, and sometimes even negative; that is, the distance will become either infinite, or more than infinite, which is absurd. And it is scarcely possible for any one certainly to determine, by means of instruments, however nice, single seconds, or even 10 seconds; and therefore, it is not at all surprising, that the exceeding minuteness of such angles has hitherto baffled the many and ingenious attempts of artists.

While I was making my observations in the island of St. Helena, about 40 years since, on the stars round the south pole, I happened to observe, with the utmost care, Mercury passing over the sun's disk: and contrary to expectation, I very accurately obtained, with a good 24-foot telescope, the very moment in which Mercury, entering the sun's limb, seemd to touch it internally, as also that of his going off; forming an angle of internal contact. Hence I discovered the precise quantity of time the whole body of Mercury had then appeared within the sun's disk, and that without an error

of one single second of time; for, the thread of solar light, intercepted between the obscure limb of the planet, and the bright limb of the sun, though exceedingly slender, affected my sight, and in the twinkling of an eye, both the indenture made on the sun's limb by Mercury entering into it, vanished, and that made by his going off, appeared. On observing this I immediately concluded, that the sun's parallax might be duly determined by such observations, if Mercury, being nearer the earth, had a greater parallax, when seen from the sun; for, this difference of parallaxes is so very inconsiderable, as to be always less than the sun's parallax, which is sought; consequently, though Mercury is to be frequently seen within the sun's disk; he will scarcely be fit for the present purpose.

There remains, therefore, Venus's transit over the sun's disk, whose parallax, being 4 times greater than that of the sun, will cause very sensible differences between the times in which Venus shall seem to pass over the sun's disk in different parts of our earth. From these differences, duly observed, the sun's parallax may be determined, even to a small part of a second of time; and that without any other instruments than telescopes and good common clocks, and without any other qualifications in the observer than fidelity and diligence, with a little skill in astronomy. For we need not be scrupulous in finding the latitude of the place, or in accurately determining the hours with respect to the meridian; it is sufficient, if the times be reckoned by clocks, truly corrected according to the revolutions of the heavens, from the total ingress of Venus on the sun's disk, to the beginning of her egress from it, when her opaque globe begins to touch the bright limb of the sun; which times, as I found by experience, may be observed even to a single second of time.

But by the limited laws of motion, Venus is very rarely seen within the sun's disk; and for a series of 120 years, and upwards, is not to be seen there once; *viz.* from 1639, when Mr. Horrox was favoured with this agreeable sight, and he the first and only one since the creation of the world, down to 1761; at which time, according to the theories hitherto observed in the heavens, Venus will pass over the sun on May 26 in the morning; so that (*vide* Phil. Trans. No. 193) at London, nearly at 6 o'clock in the morning, she is to be in the middle of the sun's disk, and but 4 minutes more southerly than his centre. The duration of this transit will be almost 8 hours; *viz.* from 2 till near 10 o'clock in the morning. Consequently her ingress will not be visible in England: but the

sun at that time being in 16° of Gemini, and almost in 23° of north declination, will be seen not to set throughout the whole northern frigid zone; and consequently the inhabitants of the coast of Norway, as far as its northern promontory, beyond the town of Drontheim, may observe Venus entering the sun's disk; and perhaps this ingress into the sun at his rising may be seen by the inhabitants of the north of Scotland and those of Zetland. But when Venus is nearest the sun's centre, he will be vertical to the northern coasts of the gulph of Ganga, or rather of the kingdom of Pegu; and consequently, in the neighbouring countries, when the sun shall, at the ingress of Venus, be almost 4 hours distant to the east, and almost as many to the west at her egress, her apparent motion within the sun's disk will be accelerated almost twice as much as in the horizontal parallax of Venus from the sun; because Venus at that time moves retrograde from east to west; while in the meantime an eye, on the surface of the earth, is carried the contrary way, from west to east.

Supposing the sun's parallax, as was said, to be 12 seconds and a half, Venus's parallax will be 43 seconds; and subtracting the sun's parallax, there will remain half a minute at least for the horizontal parallax of Venus from the sun and consequently, Venus's motion will be accelerated $\frac{3}{4}$ of a minute at least by that parallax, while she passes over the sun's disk, in such elevations of the pole as are near the Tropic; and still more so near the equator. For Venus will at that time accurately enough describe within the sun's disk 4 minutes an hour; and consequently, at least 11 minutes of time (by which the duration of this eclipse of Venus will be contracted by reason of the parallax) answer to $\frac{3}{4}$ of a minute. And by this contraction alone we might safely determine the parallax, provided the sun's diameter and Venus's latitude were very accurately given; which yet we cannot possibly bring to a calculation, in a matter of such great subtlety.

We must therefore have another observation, if possible, in places where Venus possesses the middle of the sun at midnight, *viz.* under the opposite meridian, that is, 6^h or 90° more westerly than London, and where Venus enters the sun's disk a little before his setting, and goes off a little after his rising; which will happen in the said meridian in about 56° of N. lat. that is, at Nelson's harbour in Hudson's Bay. For, in the neighbouring places Venus's parallax will protract the duration of the transit, and make it at least 6 minutes longer; because while the sun seems to tend

under the pole from west to east, these places on the earth's surface will seem to be carried with a contrary motion towards the west, that is, with a motion conspiring with the proper motion of Venus; consequently Venus will seem to move slower within the sun's disk, and continue longer on it.

If, therefore, in both places this transit happen to be duly observed by proper persons, it is evident that the Mora will be longer by 17 entire minutes in Nelson's harbour, than in the East-Indies; nor does it matter much whether the observation be made at Fort St. George, commonly called Maderas, or at Bencoolen on the western coast of the island of Sumatra near the equator. But if the French should incline to make the observation, Pondicherry on the western coast of the gulph of Ganga, at the elevation of 12°, will be a proper place for that purpose: and for the Dutch, their famous emporium Batavia is a fit place. And indeed I would have several observations made of the same phenomenon in different parts, both for further confirmation, and lest a single observer should happen to be disappointed by the intervention of clouds from seeing what I know not if those either of the present or following age shall ever see again; and upon which, the certain and adequate solution of the noblest, and otherwise most difficult problem depends. Therefore, again and again, I recommend it to the curious strenuously to apply themselves to this observation.

By this means, the sun's parallax may be discovered, to within its five hundredth part, which will doubtless seem surprising to some: but yet, if an accurate observation be had in both the places above-mentioned, it has already been shown that the duration of these eclipses of Venus differ from each other by 17 entire minutes, on the supposition that the sun's parallax is 12½ seconds. And if this difference be found to be greater or less by observation, the sun's parallax will be greater or less nearly in the same ratio.

The Detection of Proper Motions

(From "Philosophical Transactions," *Abridged*, 1717.)

Having of late had occasion to examine the quantity of the precession of the equinoctial points, I took the pains to compare the declinations of the fixed stars delivered by Ptolemy, in the 3d chapter of the 7th book of his Almag. as observed by Timocharis and Aristyllus, near 300 years before Christ, and by Hipparchus

about 170 years after them, that is about 130 years before Christ, with what we now find: and by the result of a great many calculations, I concluded that the fixed stars in 1800 years were advanced somewhat more than 25 degrees in longitude, or that the precession is somewhat more than 50″ per ann. But that with so much uncertainty, by reason of the imperfect observations of the ancients, that I have chosen in my tables to adhere to the even proportion of 5 minutes in 6 years, which from other principles we are assured is very near the truth. But while I was on this inquiry, I was surprised to find the latitudes of three of the principal stars in Heaven directly to contradict the supposed greater obliquity of the ecliptic, which seems confirmed by the latitudes of most of the rest: they being set down in the old catalogue, as if the plane of the earth's orbit had changed its situation, among the fixed stars, about 20′ since the time of Hipparchus. Particularly all the stars in Gemini are set down, those to the northward of the ecliptic, with so much less latitude than we find, and those to the southward with so much more southerly latitude. Yet the three stars Palilicium or the Bull's Eye, Sirius, and Arcturus contradict this rule directly: for by it Palilicium, being in the days of Hipparchus in about 10° of Taurus, ought to be about 15 min. more southerly than at present; and Sirius, being then in about 15° of Gemini, ought to be 20′ more southerly than now; yet on the contrary Ptolemy places the first 20′ and the other 22′ more northerly in latitude than we now find them. Nor are these errors of transcription, but are proved to be right by their declinations set down by Ptolemy, as observed by Timocharis, Hipparchus and himself, which show that those latitudes are the same as these authors intended. As to Arcturus, he is too near the equinoctial colure, to argue from him concerning the change of the obliquity of the ecliptic; but Ptolemy gives him 33′ more north latitude than he has now; and that greater latitude is likewise confirmed by the declinations delivered by the said observers. So then all these three stars are found to be above half a degree more southerly at this time than the ancients reckoned them. When, on the contrary, at the same time the bright shoulder of Orion has in Ptolemy almost a degree more southerly latitude than at present. What shall we say then? It is scarcely credible that the ancients could be deceived in so plain a matter, three observers confirming each other. Again these stars, being the most conspicuous in Heaven, are in all probability the nearest to the earth; and if they have any particular motion of

their own, it is most likely to be perceived in them, which in so long a time as 1800 years may show itself by the alteration of their places, though it be utterly imperceptible in the space of a single century of years. Yet as to Sirius, it may be observed that Tycho Brahe makes him 2′ more northerly than we now find him; whereas he ought to be above as much more southerly from his ecliptic, (the obliquity of which he makes 2½′ greater than we reckon it at present) differing in the whole 4½′. One half of this difference may perhaps be excused, if refraction were not allowed in this case by Tycho; yet 2′ in such a star as Sirius, is rather too much for him to be mistaken.

But a further and more evident proof of this change, is drawn from the observation of the application of the moon to Palilicium Anno Christi 509, March 11th, when in the beginning of the night the moon was seen to follow that star very near, and seemed to have eclipsed it. Now from the undoubted principles of astronomy, it was impossible for this to be true at Athens, or near it, unless the latitude of Palilicium were much less than we at this time find it.

But whether it were really true, that the obliquity of the ecliptic was, in the time of Hipparchus and Ptolemy, really 22′ greater than now, may well be questioned; since Pappus Alexandrinus, who lived but about 200 years after Ptolemy, makes it the very same that we do.

BRADLEY[1]

The Discovery of the Aberration of Light

(From Bradley's "Miscellaneous Works," edited by S. P. Rigaud, 1831.)

You having been pleased to express your satisfaction with what I had an opportunity sometime ago of telling you in conversation, concerning some observations that were making by our late worthy and ingenious friend, the honourable Samuel Molyneux, esq. and which have since been continued and repeated by myself, in order to determine the parallax of the fixed stars; I shall now beg leave to lay before you a more particular account of them . . .

Mr. Molyneux's apparatus was completed and fitted for observing about the end of November, 1725, and on the third day of December following, the bright star in the head of Draco (marked γ by Bayer) was for the first time observed as it passed near the zenith, and its situation carefully taken with the instrument. The like observations were made on the 5th, 11th, and 12th days of the same month, and there appearing no material difference in the place of the star, a farther repetition of them at this season seemed needless, it being a part of the year wherein no sensible alteration of parallax in this star could soon be expected. It was chiefly, therefore, curiosity that tempted me (being then at Kew, where the instrument was fixed) to prepare for observing the star on December 17th, when having adjusted the instrument as usual, I perceived that it passed a little more southerly this day than when it was observed before. Not suspecting any other cause of this appearance, we first concluded that it was owing to the uncertainty of the observations, and that either this or the foregoing were not so exact as we had before supposed; for which

[1] James Bradley (1693–1762), Astronomer Royal, accumulated accurate observations of the positions of stars, which were reduced and published in 1818 by Bessel. While attempting to measure stellar parallax, he discovered aberration and nutation. The above excerpt on aberration is from a letter to Dr. Edmond Halley and was published in the Philosophical Transactions, 1728; the excerpt on nutation is from a letter to the Rt. Hon. George Earl of Macclesfield, Dec. 31, 1747.

reason we purposed to repeat the observation again, in order to determine from whence this difference proceeded; and upon doing it on December 20th, I found that the star passed still more southerly than in the former observations. This sensible alteration the more surprised us, in that it was the contrary way from what it would have been had it proceeded from an annual parallax of the star: but being now pretty well satisfied that it could not be entirely owing to the want of exactness in the observations, and having no notion of any thing else that could cause such an apparent motion as this in the star, we began to think that some change in the materials &c. of the instrument itself might have occasioned it. Under these apprehensions we remained some time, but being at length fully convinced, by several trials, of the great exactness of the instrument, and finding by the gradual increase of the star's distance from the pole, that there must be some regular cause that produced it; we took care to examine nicely, at the time of each observation, how much it was: and about the beginning of March, 1726, the star was found to be 20″ more southerly than at the time of the first observation. It now indeed seemed to have arrived at its utmost limit southward, because in several trials made about this time, no sensible difference was observed in its situation. By the middle of April it appeared to be returning back again towards the north; and about the beginning of June, it passed at the same distance from the zenith as it had done in December, when it was first observed.

From the quick alteration of the star's declination about this time (it increasing a second in three days), it was concluded that it would now proceed northward, as it before had gone southward of its present situation; and it happened as was conjectured: for the star continued to move northward till September following, when it again became stationary, being then near 20″ more northerly than in June, and no less than 39″ more northerly than it was in March. From September the star returned towards the south, till it arrived in December to the same situation it was in at that time twelve months, allowing for the difference of declination on account of the precession of the equinox.

This was a sufficient proof that the instrument had not been the cause of this apparent motion of the star, and to find one adequate to such an effect seemed a difficulty. A nutation of the earth's axis was one of the first things that offered itself upon this occasion, but it was soon found to be insufficient; for though it

might have accounted for the change of declination in γ Draconis, yet it would not at the same time agree with the phænomena in other stars; particularly in a small one almost opposite in right ascension to γ Draconis, at about the same distance from the north pole of the equator: for though this star seemed to move the same way as a nutation of the earth's axis would have made it, yet, it changing its declination but about half as much as γ Draconis in the same time (as appeared upon comparing the observations of both made upon the same days, at different seasons of the year), this plainly proved that the apparent motion of the stars was not occasioned by a real nutation, since, if that had been the cause, the alteration in both stars would have been near equal.

The great regularity of the observations left no room to doubt but that there was some regular cause that produced this unexpected motion, which did not depend on the uncertainty or variety of the seasons of the year. Upon comparing the observations with each other, it was discovered that in both the forementioned stars, the apparent difference of declination from the maxima was always nearly proportional to the versed sine of the sun's distance from the equinoctial points. This was an inducement to think that the cause, whatever it was, had some relation to the sun's situation with respect to those points. But not being able to frame any hypothesis at that time sufficient to solve all the phænomena, and being very desirous to search a little farther into this matter, I began to think of erecting an instrument for myself at Wansted, that, having it always at hand, I might with the more ease and certainty inquire into the laws of this new motion. The consideration likewise of being able by another instrument to confirm the truth of the observations hitherto made with Mr. Molyneux's was no small inducement to me, but the chief of all was, the opportunity I should thereby have of trying in what manner other stars were affected by the same cause, whatever it was. For Mr. Molyneux's instrument, being originally designed for observing γ Draconis (in order, as I said before, to try whether it had any sensible parallax), was so contrived as to be capable of but little alteration in its direction, not above seven or eight minutes of a degree: and there being few stars within half that distance from the zenith of Kew bright enough to be well observed, he could not, with his instrument, thoroughly examine how this cause affected stars differently situated with respect to the equinoctial and solstitial points of the ecliptic . . .

My instrument being fixed, I immediately began to observe such stars as I judged most proper to give me light into the cause of the motion already mentioned. There was variety enough of small ones; and not less than twelve that I could observe through all the seasons of the year; they being bright enough to be seen in the day-time, when nearest the sun. I had not been long observing, before I perceived that the notion we had before entertained of the stars being farthest north and south, when the sun was about the equinoxes, was only true of those that were near the solstitial colure: and after I had continued my observations a few months, I discovered what I then apprehended to be a general law, observed by all the stars, *viz.* that each of them became stationary, or was farthest north or south, when they passed over my zenith at six of the clock, either in the morning or evening. I perceived likewise, that whatever situation the stars were in with respect to the cardinal points of the ecliptic, the apparent motion of every one tended the same way, when they passed my instrument about the same hour of the day or night; for they all moved southward, while they passed in the day, and northward in the night; so that each was farthest north when it came about six of the clock in the evening, and farthest south when it came about six in the morning.

Though I have since discovered that the maxima in most of these stars do not happen exactly when they come to my instrument at those hours, yet not being able at that time to prove the contrary, and supposing that they did, I endeavoured to find out what proportion the greatest alterations of declination in different stars bore to each other; it being very evident that they did not all change their declination equally. I have before taken notice that it appeared from Mr. Molyneux's observations, that γ Draconis altered its declination about twice as much as the forementioned small star almost opposite to it; but examining the matter more particularly, I found that the greatest alteration of declination in these stars was as the sine of the latitude of each respectively. This made me suspect that there might be the like proportion between the maxima of other stars; but finding that the observations of some of them would not perfectly correspond with such an hypothesis, and not knowing whether the small difference I met with might not be owing to the uncertainty and error of the observations, I deferred the farther examination into the truth of this hypothesis, till I should be furnished with a series of observa-

tions made in all parts of the year; which might enable me not only to determine what errors the observations are liable to, or how far they may safely be depended upon; but also to judge whether there had been any sensible change in the parts of the instrument itself.

Upon these considerations I laid aside all thoughts at that time about the cause of the forementioned phænomena, hoping that I should the easier discover it, when I was better provided with proper means to determine more precisely what they were.

When the year was completed, I began to examine and compare my observations, and having pretty well satisfied myself as to the general laws of the phænomena, I then endeavoured to find out the cause of them. I was already convinced that the apparent motion of the stars was not owing to a nutation of the earth's axis. The next thing that offered itself was an alteration in the direction of the plumb-line, with which the instrument was constantly rectified; but this upon trial proved insufficient. Then I considered what refraction might do; but here also nothing satisfactory occurred. At last I conjectured that all the phænomena, hitherto mentioned, proceeded from the progressive motion of light and the earth's annual motion in its orbit. For I perceived that, if light was propagated in time, the apparent place of a fixed object would not be the same when the eye is at rest, as when it is moving in any other direction than that of the line passing through the eye and object; and that when the eye is moving in different directions, the apparent place of the object would be different . . .

I must confess to you, that the agreement of the observations with each other, as well as with the hypothesis, is much greater than I expected to find before I had compared them; and it may possibly be thought to be too great by those who have been used to astronomical observations, and know how difficult it is to make such as are in all respects exact. But if it would be any satisfaction to such persons (till I have an opportunity of describing my instrument and the manner of using it), I could assure them, that in above seventy observations which I made of this star in a year, there is but one (and that is noted as very dubious on account of clouds) which differs from the foregoing hypothesis, more than 2″, and this does not differ 3″.

This, therefore, being the fact, I cannot but think it very probable that the phænomena proceed from the cause I have assigned, since the foregoing observations make it sufficiently evident, that

the effect of the real cause, whatever it is, varies in this star, in the same proportion that it ought according to the hypothesis . . .

I think it needless to give you the comparison between the hypothesis and the observations of any more stars; since the agreement in the foregoing is a kind of demonstration (whether it be allowed that I have discovered the real cause of the phænomena or not) that the hypothesis gives at least the true law of the variation of declination in different stars, with respect to their different situations and aspects with the sun. And if this is the case, it must be granted that the parallax of the fixed stars is much smaller than hath been hitherto supposed by those who have pretended to deduce it from their observations. I believe that I may venture to say, that in either of the two stars last mentioned it does not amount to 2″. I am of opinion, that if it were 1″ I should have perceived it, in the great number of observations that I made, especially of γ Draconis; which agreeing with the hypothesis (without allowing any thing for parallax) nearly as well when the sun was in conjunction with, as in opposition to, this star, it seems very probable that the parallax of it is not so great as one single second; and consequently that it is above 400,000 times farther from us than the sun.

There appearing, therefore, after all no sensible parallax in the fixed stars, the Anti-Copernicans have still room on that account to object against the motion of the earth; and they may have (if they please) a much greater objection against the hypothesis by which I have endeavoured to solve the forementioned phænomena, by denying the progressive motion of light, as well as that of the earth.

But as I do not apprehend that either of these postulates will be denied me by the generality of the astronomers and philosophers of the present age; so I shall not doubt of obtaining their assent to the consequences which I have deduced from them, if they are such as have the approbation of so great a judge of them as yourself.

The Discovery of Nutation

The great exactness with which instruments are now constructed hath enabled the astronomers of the present age to discover several changes in the positions of the heavenly bodies, which, by reason of their smallness, had escaped the notice of their predecessors.

And although the causes of such motions have always subsisted, yet philosophers had not so fully considered what the effects of those known causes would be, as to demonstrate *a priori* the phænomena they might produce; so that theory itself is here, as well as in many other cases, indebted to practice, for the discovery of some of its most elegant deductions. This points out to us the great advantage of cultivating this, as well as every other branch of natural knowledge, by a regular series of observations and experiments . . .

It appeared from my observations, that, during this interval of time, some of the stars near the solstitial colure had changed their declinations 9″ or 10″ less than a precession of 50″ would have produced; and, at the same time, that others near the equinoctial colure had altered theirs about the same quantity more than a like precession would have occasioned: the north pole of the equator seeming to have approached the stars, which come to the meridian with the sun, about the vernal equinox and the winter solstice; and to have receded from those which come to the meridian with the sun about the autumnal equinox and the summer solstice.

When I considered these circumstances, and the situation of the ascending node of the moon's orbit, at the time when I first began my observations; I suspected that the moon's action upon the equatorial parts of the earth might produce these effects: for if the precession of the equinox be, according to Sir Isaac Newton's principles, caused by the actions of the sun and moon upon those parts, the plane of the moon's orbit being at one time above ten degrees more inclined to the plane of the equator than at another, it was reasonable to conclude, that the part of the whole annual precession, which arises from her action, would in different years be varied in its quantity; whereas the plane of the ecliptic, wherein the sun appears, keeping always nearly the same inclination to the equator, that part of the precession which is owing to the sun's action may be the same every year: and from hence it would follow, that although the mean annual precession, proceeding from the joint actions of the sun and moon, were 50″, yet the apparent annual precession might sometimes exceed, and sometimes fall short of that mean quantity, according to the various situations of the nodes of the moon's orbit . . .

By the mean of 100 observations taken before the end of the year 1728, the mean distance of γ Draconis was 79″8 south of

38°25′ on March 27th, 1727; and by the mean of 35 observations, the 35th Camelopard.Hevel. was south of the same spot 76″4. So that the mean polar distance of γ Draconis was only 3″4 greater than that of the 35th Camelopard.Hevel.; but as the equation for the nutation in both these stars was then near the maximum, and to be applied with contrary signs, the apparent polar distance of γ Draconis was 21″4 greater on the 27th day of March 1727.

The differences of the polar distances of the stars, as here set down, may be presumed, both on account of the radius of the instrument and the number of observations, to be very exactly determined, to the time when the moon's ascending node was at the beginning of Aries; and if a like comparison be hereafter made, of observations taken of the same stars, near the same position of the moon's nodes, future astronomers may be enabled to settle the quantity of the mean precession of the equinox, so far as it affects the declination of these stars, with great certainty; and they may likewise discover, by means of the stars near the solstitial colure, from what cause the apparent change in the obliquity of the ecliptic really proceeds, if the mean obliquity be found to diminish gradually.

On the Real Motions of the Stars.—The forementioned points indeed can be settled only on the supposition that the angular distances of these stars do continue always the same, or that they have no real motion in themselves, but are at rest in absolute space. A supposition which, though usually made by astronomers, nevertheless seems to be founded on too uncertain principles to be admitted in all cases. For if a judgment may be formed, with regard to this matter, from the result of the comparison of our best modern observations with such as were formerly made, with any tolerable degree of exactness; there appears to have been a real change in the position of some of the fixed stars with respect to each other; and such as seems independent of any motion in our own system, and can only be referred to some motion in the stars themselves. Arcturus affords a strong proof of this; for if its present declination be compared with its place, as determined either by Tycho or Flamsteed, the difference will be found to be much greater than what can be suspected to arise from the uncertainty of their observations.

It is reasonable to expect that other instances of the like kind must also occur among the great number of the visible stars, because their relative positions may be altered by various means.

For if our own solar system be conceived to change its place with respect to absolute space, this might, in process of time, occasion an apparent change in the angular distances of the fixed stars, and, in such a case, the places of the nearest stars being more affected than of those that are very remote, their relative positions might seem to alter, though the stars themselves were really immoveable. And on the other hand, if our own system be at rest, and any of the

Hevelius' 150-foot Telescope.

stars really in motion, this might likewise vary their apparent positions; and the more so, the nearer they are to us, or the swifter their motions are, or the more proper the direction of the motion is, to be rendered perceptible by us. Since then the relative places of the stars may be changed from such a variety of causes, considering that amazing distance at which it is certain some of them are placed, it may require the observations of many ages to determine the laws of the apparent changes even of a single star; much more difficult therefore must it be to settle the laws relating to all the most remarkable stars.

When the causes which affect the places of all the stars in general are known, such as the precession, aberration, and nutation, it may be of singular use to examine nicely the relative situations of particular stars; and especially of those of the greatest lustre, which it may be presumed lie nearest to us, and may, therefore, be subject to more sensible changes, either from their own motion, or from that of our system. And if at the same time that the brighter stars are compared with each other, we likewise determine the relative positions of some of the smallest that appear near them, whose places can be ascertained with sufficient exactness, we may perhaps be able to judge to what cause the change, if any be observable, is owing. The uncertainty that we are at present under, with respect to the degree of accuracy wherewith former astronomers could observe, makes us unable to determine several things relating to the subject that I am now speaking of; but the improvements which have of late years been made, in the methods of taking the places of the heavenly bodies, are so great, that a few years may hereafter be sufficient to settle some points, which cannot now be settled by comparing even the earliest observations with those of the present age.

THOMAS WRIGHT[1]

Speculations on the Structure of the Milky Way

(From "An Original Theory of the Universe," 1750.)

This is the great Order of Nature which I shall now endeavour to prove, and thereby solve the Phænomena of the *Via Lactea;* and in order thereto, I want nothing to be granted but what may easily be allowed, namely, that the *Milky Way* is formed of an infinite Number of small Stars.

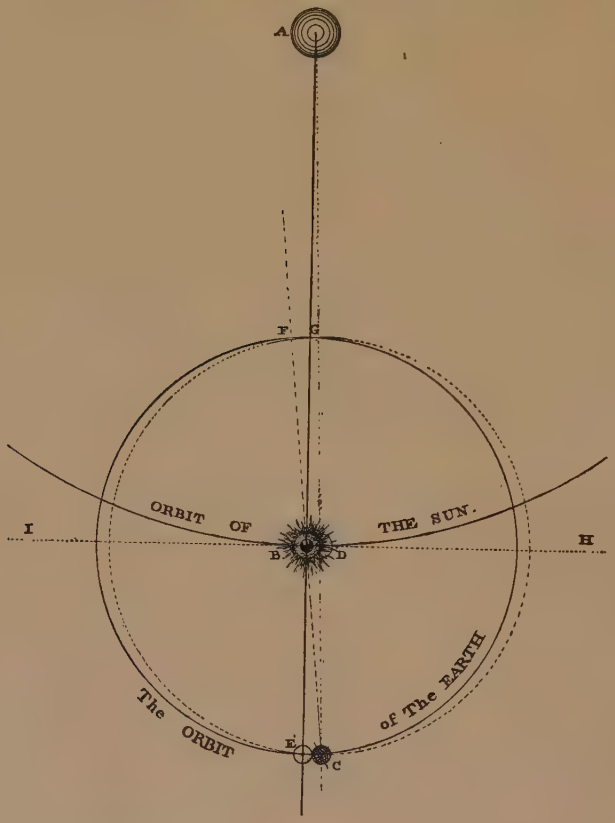
Let us imagine a vast infinite Gulph, or Medium, every Way extended like a Plane, and enclosed between two Surfaces, nearly even on both Sides, but of such a Depth or Thickness as to occupy a Space equal to the double Radius, or Diameter of the visible Creation, that is to take in one of the smallest Stars each Way, from the middle Station, perpendicular to the Plane's Direction, and, as near as possible, according to our Ideas of their true Distance.

But to bring this Image a little lower, and as near as possible level to every Capacity, I mean such as cannot conceive this kind of continued Zodiack, let us suppose the whole Frame of Nature in the Form of an artificial Horizon of a Globe, I don't mean to affirm that it really is so in Fact, but only state the Question thus, to help your Imagination to conceive more aptly what I would explain.[2] The figure will then represent a just Section of it. Now in this Space let us imagine all the Stars scattered promiscuously, but at such an adjusted Distance from one another, as to fill up the whole Medium with a kind of regular Irregularity of Objects. And next let us consider what the Consequence would be to an Eye situated near the Center Point, or any where about the middle Plane, as at the Point *A*. Is it not, think you, very

[1] Thomas Wright (1711–1786), of Durham, England, was the first to give a definite theory of the structure of the Milky Way. He proposed the "grindstone" hypothesis of the form of the stellar universe, later developed by Sir William Herschel.

[2] Not to mention their several Conjunctions and Apulces to fixed Stars, &c., See the State of the Heavens in 1662, *December* the first, when all the known Planets were in one Sign of the Zodiac, *viz. Sagittarius.*

evident, that the Stars would there appear promiscuously dispersed on each Side, and more and more inclining to Disorder, as the Observer would advance his Station towards either Surface, and nearer to *B* or *C*, but in the Direction of the general Plane towards *H* or *D*, by the continual Approximation of the visual Rays, crowding together as at *H*, betwixt the Limits *D* and *G*,



they must infallibly terminate in the utmost Confusion. If your Opticks fails you before you arrive at these external Regions, only imagine how infinitely greater the Number of Stars would be in those remote Parts, arising thus from their continual crowding behind one another, as all other Objects do towards the Horizon Point of their Perspective, which ends but with Infinity: Thus, all their Rays at last so near uniting, must meeting in the Eye appear, as almost, in Contact, and form a perfect Zone of Light; this I take

to be the real Case, and the true Nature of our *Milky Way*, and all the Irregularity we observe in it at the Earth, I judge to be entirely owing to our Sun's Position in this great Firmament, and may easily be solved by his Excentricity, and the Diversity of Motion that may naturally be conceived amongst the Stars themselves, which may here and there, in different Parts of the Heavens, occasion a cloudy Knot of Stars, as perhaps at *E*.

But now to apply this Hypothesis to our present Purpose, and reconcile it to our Ideas of a circular Creation, and the known Laws of orbicular Motion, so as to make the Beauty and Harmony of the Whole consistent with the visible Order of its Parts, our Reason must now have recourse to the Analogy of Things. It being once agreed, that the Stars are in Motion, which, as I have endeavoured in my last Letter to shew is not far from an undeniable Truth, we must next consider in what Manner they move. First then, to suppose them to move in right Lines, you know is contrary to all the Laws and Principles we at present know of; and since there are but two Ways that they can possibly move in any natural Order, that is, either in right Lines, or in Curves, this being one, it must of course be the other, *i.e.* in an Orbit; and consequently, were we able to view them from their middle Position, . . . we might expect to find them separately moving in all manner of Directions round a general Center, such as is there represented. It only now remains to shew how a Number of Stars, so disposed in a circular Manner round any given Center, may solve the Phænomena before us. There are but two Ways possible to be proposed by which it can be done, and one of which I think is highly probable; but which of the two will meet your Approbation, I shall not venture to determine, only here inclosed I intend to send you both. The first is in the Manner I have above described, *i.e.* all moving the same Way, and not much deviating from the same Plane, as the Planets in their heliocentric Motion do round the solar Body . . .

The second Method of solving this Phænomena, is by a spherical Order of the Stars, all moving with different Direction round one common Center, as the Planets and Comets together do round the Sun, but in a kind of Shell, or concave Orb. The former is easily conceived, from what has been already said, and the latter is as easy to be understood, if you have any Idea of the Segment of a Globe . . . not only the Phænomena of the *Milky Way* may be thus accounted for, but also all the cloudy Spots, and irregular

Distribution of them; and I cannot help being of Opinion, that could we view *Saturn* thro' a Telescope capable of it, we should find his Rings no other than an infinite Number of lesser Planets, inferior to those we call his Satellites: What inclines me to believe it, is this, this Ring, or Collection of small Bodies, appears to be sometimes very excentric, that is, more distant from *Saturn's* Body on one Side than on the other, and as visibly leaving a larger Space between the Body and the Ring; which would hardly be the Case, if the Ring, or Rings, were connected, or solid, since we have good Reason to suppose, it would be equally attracted on all Sides by the Body of *Saturn*, and by that means preserve everywhere an equal Distance from him; but if they are really little Planets, it is clearly demonstrable from our own in like Cases, that there may be frequently more of them on one Side, than on the other, and but very rarely, if ever, an equal Distribution of them all round the *Saturnian* Globe.

KANT[1]

On the Origin of the World

(From "Universal Natural History and Theory of the Heavens," 1755, in Hastie's "Kant's Cosmogony," 1900.)

I assume that all the material of which the globes belonging to our solar system—all the planets and comets—consist, at the beginning of all things was decomposed into its primary elements, and filled the whole space of the universe in which the bodies formed out of it now revolve. This state of nature, when viewed in and by itself without any reference to a system, seems to be the very simplest that can follow upon nothing. At that time nothing had yet been formed. The construction of heavenly bodies at a distance from each other, their distances regulated by their attractions, their form arising out of the equilibrium of their collected matter, exhibit a later state. The state of nature which immediately bordered on the creation was as crude, as unformed, as possible. But even in the essential properties of the elements that constituted this chaos, there could be traced the mark of that perfection which they have derived from their origin, their essential character being a consequence of the eternal idea of the Divine Intelligence. The simplest and most general properties which seem to be struck out without design, the matter which appears to be merely passive and wanting form and arrangement, has in its simplest state a tendency to fashion itself by a natural evolution into a more perfect constitution. But the variety in the kinds of elements, is what chiefly contributes to the stirring of nature and

[1] Immanuel Kant (1724–1804), one of the greatest philosophers of all times, a professor in the University of Königsberg, was a pioneer in the interpretation of the sidereal universe. He proposed many of the hypotheses which have been but recently restated or demonstrated, including the island universe interpretation of spiral nebulæ, the displacement of the Sun to the north of the plane of the Milky Way, and the slowing down of the Earth's rotation through tidal friction arising from the Moon's attraction. His most significant contribution was the speculation given above on the origin of the planetary system—a nebular hypothesis that preceded the better known Laplacian theory by forty years.

to the formative modification of chaos, as it is by it that the repose which would prevail in a universal equality among the scattered elements is done away, so that the chaos begins to take form at the points where the more strongly attracting particles are. The kinds of this elementary matter are undoubtedly infinitely different, in accordance with the immensity which nature shows on all sides. Those elements, which are of greater specific density and force of attraction, and which of themselves occupy less room and are also rarer, would therefore be more scattered than the lighter kinds when the material of the world was equally diffused in space. Elements of a thousand times greater specific gravity, would, therefore, be thousands or even millions of times more scattered than those that are lighter in that proportion. And as these gradations must be thought to be as infinite as possible, there may be material particles of a kind which exceed those of another in density in the same proportion as a globe described with the radius of the planetary system does another which has only the thousandth part of a line in diameter; and thus these kinds of scattered elements would be separated from each other by a distance as great as those globes themselves.

In a region of space filled in this manner, a universal repose could last only a moment. The elements have essential forces with which to put each other in motion, and thus are themselves a source of life. Matter immediately begins to strive to fashion itself. The scattered elements of a denser kind, by means of their attraction, gather from a sphere around them all the matter of less specific gravity; again, these elements themselves, together with the material which they have united with them, collect in those points where the particles of a still denser kind are found; these in like manner join still denser particles, and so on. If we follow in imagination this process by which nature fashions itself into form through the whole extent of chaos, we easily perceive that all the results of the process would consist in the formation of diverse masses which, when their formation was complete, would by the equality of their attraction be at rest and be for ever unmoved.

But nature has other forces in store, which are especially exerted when matter is decomposed into fine particles. They are those forces by which these particles repel each other, and which, by their conflict with attraction, bring forth that movement which is, as it were, the lasting life of nature. This force of repulsion is manifested in the elasticity of vapours, the effluences of strong

smelling bodies, and the diffusion of all spirituous matters. This force is an incontestable phenomenon of matter. It is by it that the elements, which may be falling to the point attracting them, are turned sideways promiscuously from their movement in a straight line; and their perpendicular fall thereby issues in circular movements, which encompass the centre towards which they were falling. In order to make the formation of the world more distinctly conceivable, we will limit our view by withdrawing it from the infinite universe of nature and directing it to a particular system, as the one which belongs to our sun. Having considered the generation of this system, we shall be able to advance to a similar consideration of the origin of the greater world-systems, and thus to embrace the infinitude of the whole creation in one conception.

From what has been said, it will appear that if a point is situated in a very large space where the attraction of the elements there situated acts more strongly than elsewhere, then the matter of the elementary particles scattered throughout the whole region will fall to that point. The first effect of this general fall is the formation of a body at this centre of attraction which, so to speak, grows from an infinitely small nucleus by rapid strides; and in the proportion in which this mass increases, it also draws with greater force the surrounding particles to unite with it. When the mass of this central body has grown so great that the velocity with which it draws the particles to itself from great distances, is bent sideways by the feeble degrees of repulsion with which they impede each other, and when it issues in lateral movements which are capable by means of the centrifugal force of encompassing the central body in an orbit, then there are produced whirls or vortices of particles, each of which by itself describes a curved line by the composition of the attracting force and the force of revolution that has been bent sideways. These kinds of orbits all intersect each other, for which their great dispersion in this space gives place. Yet these movements are in many ways in conflict with each other, and they naturally tend to bring one another to a uniformity, that is, into a state in which one movement is as little obstructive to the other as possible. This happens in two ways: first, by the particles limiting each other's movement till they all advance in one direction, and secondly, in this way, that the particles limit their vertical movements in virtue of which they are approaching the centre of attraction, till they all moving horizontally, *i.e.* in parallel circles round the sun as their centre, no

longer intersect each other, and by the centrifugal force becoming equal with the falling force they keep themselves constantly in free circular orbits at the distance at which they move. The result, finally, is that only those particles continue to move in this region of space which have acquired by their fall a velocity, and through the resistance of the other particles a direction, by which they can continue to maintain a *free circular movement*. In this state, when all the particles are moving in one direction and in parallel circles, *i.e.* in free circular movements carried on by the acquired propulsive forces around the central body, the conflict and the concourse of the elements is annulled, and everything is then in the state of the least reciprocal action. This state is the natural consequence which always ensues in the case of matter involved in conflicting movements. It is therefore clear that a great number of the scattered multitude of particles must attain to such exact determinate conditions through the resistance by which they seek to bring each other to this state; although a much greater multitude of them do not reach it and only serve to increase the mass of the central body into which they fall, as they cannot maintain themselves freely at the distance at which they are moving, but cross the circles of the nearer particles, and, finally, by their resistance lose all motion. This body in the centre of attraction which, in consequence of all this, has become the chief part of the planetary system by the mass of its collected matter, is the sun, although it has not yet that glow of flame which bursts out on its surface after its formation has become entirely complete.

It is further to be observed, that all the elements of nature, when fashioning itself, thus move, as has been shown, in a direction round the centre of the sun; and hence in these revolutions, which are directed to a single region and which are performed as it were upon a common axis, the rotation of the fine matter cannot proceed in this way. Because, according to the laws of centrifugal motion, all the revolutions must intersect the centre of attraction with the plane of their orbits; but of all these orbits round a common axis that move in one direction, there is only one which cuts through the centre of the sun. Hence all the matter on both sides of this imaginary axis hurries to that circle which passes through the rotation of the axis in the centre of the common attraction, which circle is the plane of the reference of all the revolving elements: around which plane they accumulate as much as possible, and contrariwise leave the regions at a distance from this plane empty.

For those elements which cannot come so near this plane to which they are all pressing, will not be able to continue to maintain themselves in the places where they are moving, but, impinging on the elements floating around them, this will cause them finally to fall into the sun.

If we, therefore, consider this revolving elementary matter of the world in that state into which it puts itself by attraction and by the mechanical consequence of the general laws of resistance, we see a region of space extending from the centre of the sun to unknown distances, contained between two planes not far distant from each other, in the middle of which the general plane of reference is situated. And this elementary matter is diffused in this space within which all the contained particles—each according to the proportion of its distance and of the attraction which prevails there—perform regulated circular movements in free revolutions. And hence, as in this arrangement they obstruct each other as little as possible, they would continue always in this relation, if the attraction of the elementary matters for each other did not then begin to produce its effect, and thereby give occasion to new formations which are the seed of the planets that are about to arise. For the elements which move round the sun in parallel circles and not at too great a difference of distance from the sun, are by the equality of their parallel motion almost at rest respectively towards each other; and thus the attraction of those elements there which are of higher specific attraction immediately produces an important effect, namely, the collecting of the nearest particles for the formation of a body which according to the proportion of the growth of its mass, extends its attraction farther and draws elements from a wide region to unite with it in its further formation.

The view of the formation of the planets in this system has the advantage over every other possible theory in holding that the origin of the masses gives the origin of the movements, and the position of the orbits as arising at the same point of time; nay more, in showing that even the deviations from the greatest possible exactness in these determinations, as well as the accordances themselves, become clear at a glance. The planets are formed out of particles which at the distance at which they move, have exact movements in circular orbits; *and therefore the masses composed out of them will continue the same movements at the same rate and in the same direction.* This suffices to show why the motion of the planets is almost circular, and why their orbits are in one plane.

They would be exact circles if the range out of which they collected the elements for their formation was very small, and then the difference of their movements would be very slight. But as a wide range of space is required in order that the dense mass of a planet may be formed out of the fine stuff which is so widely diffused in the celestial space, the difference of the distances which these elements have from the sun, and consequently also the difference of their velocities, is no longer insignificant. In consequence, it would be necessary, in order to maintain in the planet the equilibrium of the centripetal forces and the circular velocity with this difference of movements, that the particles which come together upon it from different distances with different movements should exactly compensate for the defects of each other. And although this really takes place with tolerable exactness, nevertheless, as it falls somewhat short of a perfect compensation, the result is divergence from the circular movement and eccentricity. It is as easily seen that, although the orbits of all the planets ought properly to be in one plane, yet even in this respect a small deviation is presented, because, as already mentioned, the elementary particles while situated as near as possible to the general plane of their movements, yet occupy some space on both sides of it. Now it would be a very happy chance if all the planets were to begin to form themselves exactly in the middle between these two sides in their relative plane, which already would bring about some inclination of their orbits to each other, although the tendency of the particles to limit this divergence on both sides, permits it only within very narrow limits. It is, therefore, not astonishing that the greatest exactness of determination is not to be found here any more than in any of the other products of nature, because the multiplicity of the circumstances which enter into every fact of nature does not admit of absolute regularity.

Island Universes

It is far more natural and conceivable to regard them [nebulæ] as being not such enormous single stars but systems of many stars, whose distance presents them in such a narrow space that the light which is individually imperceptible from each of them, reaches us, on account of their immense multitude, in a uniform pale glimmer. Their analogy with the stellar system in which we find

ourselves, their shape, which is just what it ought to be according to our theory, the feebleness of their light which demands a presupposed infinite distance: all this is in perfect harmony with the view that these elliptical figures are just universes and, so to speak, Milky Ways, like those whose constitution we have just unfolded. And if conjectures, with which analogy and observation perfectly agree in supporting each other, have the same value as formal proofs, then the certainty of these systems must be regarded as established.

The attention of the observers of the heavens, has thus motives enough for occupying itself with this subject. The fixed stars, as we know, are all related to a common plane and thereby form a co-ordinated whole, which is a World of worlds. We see that at immense distances there are more of such star-systems, and that the creation in all the infinite extent of its vastness is everywhere systematic and related in all its members.

It might further be conjectured that these higher universes are not without relation to one another, and that by this mutual relationship they constitute again a still more immense system. In fact, we see that the elliptical figures of these species of nebulous stars, as represented by M. de Maupertuis, have a very near relation to the plane of the Milky Way. Here a wide field is open for discovery, for which observation must give the key. The Nebulous Stars, properly so called, and those about which there is still dispute as to whether they should be so designated, must be examined and tested under the guidance of this theory. When the parts of nature are considered according to their design and a discovered plan, there emerge certain properties in it which are otherwise overlooked and which remain concealed when observation is scattered without guidance over all sorts of objects.

The theory which we have expounded opens up to us a view into the infinite field of creation, and furnishes an idea of the work of God which is in accordance with the infinity of the great Builder of the universe. If the grandeur of a planetary world in which the earth, as a grain of sand, is scarcely perceived, fills the understanding with wonder; with what astonishment are we transported when we behold the infinite multitude of worlds and systems which fill the extension of the Milky Way! But how is this astonishment increased, when we become aware of the fact that all these immense orders of star-worlds again form but one of a number whose termination we do not know, and which perhaps, like the former, is a

system inconceivably vast—and yet again but one member in a new combination of numbers! We see the first members of a progressive relationship of worlds and systems; and the first part of this infinite progression enables us already to recognize what must be conjectured of the whole. There is here no end but an abyss of a real immensity, in presence of which all the capability of human conception sinks exhausted, although it is supported by the aid of the science of number. The Wisdom, the Goodness, the Power which have been revealed is infinite; and in the very same proportion are they fruitful and active. The plan of their revelation must therefore, like themselves, be infinite and without bounds.

A Discussion of the Earth's Axial Rotation

(From "Preliminary Discussion," 1754, in Hastie's "Kant's Cosmogony," 1900.)

If the Earth were a wholly solid mass without any fluid elements, neither the attraction of the sun nor of the moon would do anything to alter its free axial rotation. For it draws the eastern as well as the western parts of the terrestrial globe with the same force, and causes thereby no inclination either to the one side or the other, and consequently it leaves the earth in complete freedom to continue its rotation unhindered, just as if it were subject to no external influence. But taking it to be the case that the mass of the planet comprises in it a considerable quantity of the fluid element, then the combined attractions of the moon and the sun, by moving this fluid matter, will impress on the earth a part of this agitation. The earth is actually found in such circumstances. The water of the ocean covers at least a third of its surface, and by the attraction of the said heavenly bodies it is kept in ceaseless motion—and it moves towards that side which is directed right opposite to the axial rotation. It deserves, therefore, to be carefully considered, whether this cause is not capable of bringing about some alteration of the rotation. The attraction of the moon, which has the greatest share in this effect, keeps the water of the ocean swelling incessantly, whereby it strives to flow to the points directly under the moon, both on the side turned to it and on that which is turned away from it, and thus to rise up; and because these points in the swell advance from east to west, they communi-

cate to the ocean a constant onflow of all it contains in that direction. The experience of navigators has long since put this universal motion beyond doubt; and it is observed most distinctly in gulfs and bays, where the water increases its velocity from having necessarily to course through a narrow strait. Now, as this onward flow is directly opposed to the rotation of the earth, we have a cause upon which we can certainly calculate that it is incessantly exerted with all its power to weaken and to diminish that rotation.

It is true that if we compare the slowness of this motion with the rapidity of the earth, the slightness of the quantity of the water with the greatness of the globe, and the lightness of the former with the heaviness of the latter, it may appear that its effect might be held to be nothing. But, on the other hand, when it is considered that this rush is incessant; that it has lasted from the beginning of time, and will always go on; that the Rotation of the Earth is a free motion, in which the slightest quantity that is taken from it is lost without reparation, whereas the diminishing cause remains unceasingly active in the same strength, it would be a prejudice highly unbecoming in a philosopher to declare a slight effect to be insignificant when, by its constant summation, it must yet ultimately exhaust even the largest quantity.

LAMBERT[1]

Concerning Systems of Systems

(From "Kosmologischen Briefe," 1761; translated from the French by James Jacque, Esq., 1800.)

Before we proceed in our enquiry concerning the motion of the fixed stars, we must endeavour to have a just idea of their situation and arrangement in the heavens. While we raise our eyes to the firmament, we see the whole of the stars attached, as it were, to the same vaulted surface; this, however, is an optical illusion; they are, in reality, at very different distances from us, as well as from the Sun, which is the fixed star of our system.

In order to prove this, we shall not have recourse either to the annual paralax of the Earth, which is too inconsiderable to measure such vast distances, nor to the observed motion of the fixed stars, which the observations of several centuries would scarcely be sufficient to discover, even in a small degree.

Our evidence will be derived partly from the apparent as well as real light and magnitude of those stars, and partly from the laws of cosmogony.

One star appears larger than another, not only because it actually is so, or that it is at a smaller distance from the eye; but, likewise, because it has a greater degree of lustre; a circumstance, the reason of which we discover in the pupil of the eye, in the confusion of the image painted in the retina, and in the dispersion of the light on the same organ. Our best glasses, by extinguishing the scattered and tremulous rays, shew us the fixed stars like so many luminous points. For similar reasons, one star seems to shine with a greater brilliancy than another, especially if it has more lustre in its own nature. We may presume, also, that their light suffers a diminution in the atmospheres of the Earth and Sun, in the ether itself, in the atmosphere of other fixed stars which it traverses, and in that of the bodies which revolve round themselves. Thus, the more

[1] Johann Heinrich Lambert (1728–1777), Alsatian scientist. His outstanding work was in the fields of photometry, cometary orbit theory, and cosmogonical speculation.

distant a star is, the more pale and feeble will its light become by the time it reaches the eye; but, if all the stars were equally distant, their light would decay proportionally, and this difference would not exist.

If they were all of the same magnitude, and equally brilliant, we should thence infer, that such as appear of the smallest size are the most remote. And, were they all at an equal distance, we ought to conclude that the small ones had, at the same time, an inferior degree of brilliancy. It is observed that the number of stars in the different classes of magnitude increases nearly as the square of the term of each magnitude. There are eighteen of the first magnitude, sixty-eight of the second, two hundred and nine of the third, four hundred and fifty-three of the fourth, &c. It is certain that this progression is much better accounted for by the different distances of the stars, though we are far from contending that they are all equally large and equally luminous.

The Milky Way.—The outline of the milky way seems extremely irregular to the eye, and its breadth very unequally verging from three degrees in some places to 25 degrees in others even to 30 degrees. There are places where it appears broken, lacerated, or split into several pieces, some of which seem to overleap the general boundary. In short, taken all together, it is visibly detached from the rest of the heavens, and the number of its stars, compared with those that are without it, is like the ocean compared to a drop of water.

What then is the cause of this apparently feeble light in the stars of the milky way? For all the fixed stars being destined to serve the same end, we see no reason to believe that these have a light originally weaker than the rest. It can only be on account of their distance then, that they make a fainter impression on our senses. The milky way lies in the background of the other stars, at such an immense distance, as prevents our discovering its component stars otherwise than with a telescope. This being the case, no reason can possibly be assigned, why those stars should not be in themselves equally large and luminous with our Sun.

The circumstance of distance leads us to conceive, in like manner, that notwithstanding their apparent proximity, they may be separated from one another by vast intervals. And, in fact, everything concurs to persuade us, that there is a distance between them similar to what exists between the other fixed stars; for

example, between the Sun and Sirius, or the fixed star the most contiguous to our system . . .

But if we suppose that those stars are separated by distances equal to those that the other fixed stars hold in respect to one another, we would thence draw this other conclusion, that the stars of the milky way are arranged not in the same line, but the one behind the other in immense serieses . . .

. . . it necessarily follows that the stars in the milky way are some more, some less remote, and that they succeed each other in numberless serieses, stretching progressively into the abyss of the universe. The stars which are out of the tract of the milky way, being also, as we have shewn, at various distances from us, form similar serieses, though less extended in length.

Let us consider at present the whole visible stars in mass, and we shall perceive that this whole does not exhibit a spherical figure, but rather that of a physical plane or disk, whose diameter is much greater than the axis which measures its thickness. In this plane lie the milky way, and all that is without it: it may be regarded as the ecliptic of the other fixed stars. It represents a flattened cylinder, or a spheroid, which for a row of a hundred stars in its thickness, ought to have a train of millions in its length; and it is this that defines the general aspect, or *coup d'æil*, for we see it in an oval form.

Systems of Higher Order.—Our Earth belongs, by a chain of gradations, to several systems, and at last to the system of the universe: all the centres of those systems as well as the universal centre, exert their influence over her. The whole of those centres then, ought, in respect of the Earth, to occupy a sensible space in the heavens; at least we have a right to suppose that we ought to see them with the telescope. Nothing but the inconveniencies arising from the transmission of light as detailed above, could intercept our view of them. For as to their magnitude, it is such as it would be necessary to render them visible.

Here then we have all the systems of the universe reduced to order, and enchased in one another. But what is our position amidst those systems? Where are we? As to this point we can speak indefinitely, negatively, and by approximation only.

The Earth is not at the centre of the solar system. The Sun is not at the centre of his system of fixed stars; a centre which is either in the region of Orion or Sirius. This system is neither at the centre nor in the plane of the milky way, though it seems to

project over it a little; the portion of this way which it approaches the nearest, is that which passes by the colure of Capricorn, where its breadth is double. But where is the milky way itself in relation to other milky ways? Here ends all our science with the utmost stretch of our eyes and instruments.

On the Complication of Celestial Motions.—Hitherto we have proceeded on the supposition that the heavenly bodies revolve in elipses. The new point of view to which our theory leads us, will produce an entire change; and we shall see that we have reviewed a suite of hypotheses which overturn each other, in proportion as we advance in our enquiry.

The Moon, it is said, describes an elipse round the Earth. This would be true were the Earth at rest; but as she moves round the Sun, and obliges the Moon to participate in her motion, the orbit of this last cannot be an elipse, but a cycloid. The elipse of the Earth vanishes for the same reason, the moment the Sun ceases to be immoveable, and is found to describe an orbit round a new centre. Then the elipse of the Earth becomes a cycloid of the first degree, that of the Moon of the second, and the velocity of their motion increases in the same proportion.

But this order continues no longer than the new centre, or body of the fourth degree, reckoning from the Moon, is supposed immoveable. As soon as we give motion to it, the elipse of the sun vanishes in its turn; he then describes a cycloid of the first degree, the Earth of the second, the Moon of the third.

We easily perceive, that what is here said of the Moon, the Earth, and the Sun, applies equally to all the satellites, planets, comets, and fixed stars, without exception. There is not a heavenly body which does not partake of these motions, more or less complex; while each shares them in a degree suited to its particular circumstances.

We observe likewise, that as we pass on from centre to centre, these motions become more and more complicated; and their combinations only terminate at the universal centre, which alone is in a state of real and absolute rest. If, beginning by the Moon, we suppose that the body which occupies that centre is in the thousandth, the cycloid of the Earth will be in the nine hundred and ninety-eighth degree. There, and there alone, will be the true orbit of the Earth, while the velocity with which she describes it, will be her true velocity. But who is in condition to determine it, to describe the nature of her cycloid, to trace the perplexed path

of our planet, and the strange bounds or skips she makes in the regions of the Heavens?

Nothing, however, is more evident. The Earth as well as all the other globes revolve, properly speaking, round the universal centre alone. With respect to the Sun, she only attends him in the same route, and as his fellow traveller, avails herself of his company, by partaking in his light and heat. She undoubtedly makes many circuitous, and, as they may appear to us, useless trips, but which, as the law of gravitation supplies no other means of keeping two or more bodies together, are nevertheless necessary. She gravitates towards all the centres on which she depends; with the Moon towards the Sun, with the Sun towards a body of the fourth degree, with this last towards a body of the fifth, or towards the centre of the milky way, and so on of the rest.

Thus, from system to system, our cycloid takes new inflections, which increase in magnitude as we advance in our career.

General Conclusion.—Let us recapitulate and have done. The law of gravitation extends universally over all matter. The fixed stars obeying central forces move in orbits. The milky way comprehends several systems of fixed stars; those that appear out of the tract of the milky way form but one system which is our own. The sun being of the number of fixed stars, revolves round a centre like the rest. Each system has its centre, and several systems taken together have a common centre. Assemblages of their assemblages have likewise theirs. In fine, there is a universal centre for the whole world round which all things revolve. Those centres are not void, but occupied by opaque bodies. Those bodies may borrow their light from one or more Suns, and hence become visible with phases. Perhaps the pale light seen in Orion is our centre. The real orbits of comets, planets, and suns, are not elipses, but cycloids of different degrees. The orbits of those bodies which are immediately subject to the action of the universal centre can alone be elipses.

LAGRANGE[1]

On the Solution of the Problem of Three Bodies

(Translated from "Essai d'une Nouvelle Méthode pour Résoudre le Problème des Trois Corps," Prix de l'Académie, Tome IX, 1772.)

We shall now examine several particular cases, in which the Problem of Three Bodies is much simplified and admits an accurate or nearly accurate solution; although these cases do not exist in the system of the earth, we believe, however, that they deserve the attention of Geometricians, because from these cases light may be thrown on the general solution of the Problem of Three Bodies.

The first case which presents itself is the one where the three distances r, r^1, r^2, are constant, in such a way that the triangle formed by these bodies always remains the same, and changes only position.

[In some twenty pages of fairly simple mathematical analysis, Lagrange discusses this and other special cases of the Problem of Three Bodies and summarizes the results in the following paragraph.]

We have seen that the Problem of Three Bodies can be solved exactly in the case where the distances between the three bodies, remain constant or constant ratios of the distances are maintained; and this is possible in two cases, *i.e.*, when the three distances are equal to each other, so that the three bodies always form an equilateral triangle, and when one of the distances is equal to the sum or to the difference of the other two, so that the three bodies are always placed in a straight line.

If we suppose that the distances r, r^I, r^{II}, are variable, but that their values deviate only a very little from those which they should have for one of the preceding cases to occur, it is clear that the

[1] Joseph Louis Lagrange (1736–1813), French mathematician, while head of the mathematical section of the Berlin Academy, and Professor of Mathematics in Paris, produced an extraordinary series of papers on astronomy as well as on dynamics and mathematics. He was one of the great analysts who worked on "Newton's Problem," or the general problem of three bodies, which, together with Newton's contributions, was the foundation on which Laplace developed his "Mécanique Céleste".

problem will be practically soluble by known methods of approximation, but we shall not enter here upon this detail which would take us too far from our principal object.

Finally, I admit that the preceding problem could be solved in a simpler way by means of the ordinary formulæ of the Problem of Three Bodies, connecting the radii vectores and the angles described by these radii, if one were first willing to limit himself to the hypothesis that bodies move in the same fixed plane; but, it seems to me, it would not be easy to obtain the result by the same formulæ if one assumes, as we have, that bodies can move in different planes.

MASKELYNE[1]

The Mountain Method of Measuring the Earth's Density

(From "Philosophical Transactions," *Abridged*, 1775.)

A Proposal for Measuring the Attraction of Some Hill in This Kingdom by Astronomical Observations.—If the attraction of gravity be exerted, as Sir Isaac Newton supposes, not only between the large bodies of the universe, but between the minutest particles of which these bodies are composed, or into which the mind can imagine them to be divided, acting universally according to that law by which the force which carries on the celestial motions is regulated; namely, that the accelerative force of each particle of matter, towards every other particle, decreases as the squares of the distances increase; it will necessarily follow, that every hill must, by its attraction, alter the direction of gravitation in heavy bodies in its neighborhood, from what it would have been from the attraction of the earth alone, considered as bounded by a smooth and even surface. For, as the tendency of heavy bodies downwards, perpendicular to the earth's surface, is owing to the combined attraction of all the parts of the earth on it, so a neighboring mountain ought, though in a far less degree, to attract the heavy body towards its centre of attraction, which cannot be placed far from the middle of the mountain. Hence the plumb-line of a quadrant, or any other astronomical instrument, must be deflected from its proper situation, by a small quantity towards the mountain; and the apparent altitudes of the stars, taken with the instrument, will be altered accordingly.

It will easily be acknowledged, that to find a sensible attraction of any hill, from undoubted experiment, would be a matter of no small curiosity, would greatly illustrate the general theory of gravity, and would make the universal gravitation of matter as it were palpable, to every person, and fit to convince those who will yield their ascent to nothing but downright experiment. Nor

[1] Nevil Maskelyne (1732–1811), English astronomer, observed the transit of Venus from St. Helena in 1761, became Astronomer Royal in 1765, and in 1774 took part in the measurement of the earth's density.

would its uses end here, as it would serve to give us a better idea of the total mass of the earth, and the proportional density of the matter near the surface, compared with the mean density of the whole earth. The result of such an uncommon experiment, which I should hope would prove successful, would doubtless do honour to the nation where it was made, and the society which executed it.

Sir Isaac Newton gives us the first hint of such an attempt, in his popular Treatise of the System of the World, where he remarks, "That a mountain of a hemispherical figure, 3 miles high and 6 broad, will not, by its attraction, draw the plumb-line 2 minutes out of the perependicular." It will appear, by a very easy calculation, that such a mountain would attract the plumb-line 1′18″ from the perpendicular.

But the first attempt of this kind was made by the French academicians, who measured 3 degrees of the meridian near Quito in Peru, and who endeavoured to find the effect of the attraction of Chimboraço, a mountain in that neighborhood, which is elevated near 4 miles above the sea, though only about 2 miles above the general level of the province of Quito. By their observations of the altitudes of fixed stars, taken with a quadrant of 2½ feet radius, they found the quantity of 8″ in favour of the attraction of the mountain, by a mean of their observations. This indeed was much less than they expected; but then it is to be considered, that their instrument was too small and imperfect for the purpose; and that they themselves were subject to great inconveniencies, being sheltered from the wind and weather by nothing but a common tent, and placed so high up the mountain as the boundary where the snow begins to lie unmelted all the year round. And indeed their observations, doubtless owing to these causes of error, differ greatly from each other, and are therefore insufficient to prove the reality of an attraction of the mountain Chimboraço, though the general result from them is in favour of it . . .

An Account of Observations Made on the Mountain Schehallien for Finding Its Attraction[1].— . . .

Perthshire afforded a remarkable hill, nearly in the centre of Scotland, of sufficient height, tolerably detached from other hills, and considerably larger from east to west than from north to

[1] For this paper, Dr. Maskelyne was honoured with the Society's gold medal. And the calculation of the earth's density, from these observations, amply confirmed the expectations and predictions of it; as fully appears in a future volume of this work.

south, called by the people of the low country Maiden-pap, but by the neighboring inhabitants, Schehallien; which, I have since been informed, signifies in the Erse language, constant storm; a name well adapted to the appearance which it so frequently exhibits to those who live near it, by the clouds and mists which usually crown its summit. It had also the advantage, by its steepness, of having but a small base from north to south; which circumstance, at the same time that it increases the effect of attraction, brings the two stations on the north and south sides of the hill, at which the sum of the two contrary attractions is to be found by the experiment, nearer together; so that the necessary allowance of the number of seconds, for the difference of latitude due to the measured horizontal distance of the two stations, in the direction of the meridian, would be very small, and consequently not subject to sensible error from any probable uncertainty of the length of a degree of latitude in this parallel. For these reasons the mountain Schehallien was chosen, in preference to all others, for the scene of the intended operations, and it was concluded to make the experiment in the summer of the year 1774 . . .

The quantity of attraction of the hill, the grand point to be determined, is measured by the deviation of the plumb-line from the perpendicular, occasioned by the attraction of the hill, or by the angle contained between the actual perpendicular and that which would have obtained if the hill had been away. The meridian zenith distances of fixed stars, near the zenith, taken with a zenith sector, being of all observations hitherto devised capable of the greatest accuracy, ought by all means to be made use of on this occasion: and it is evident, that the zenith instrument should be placed directly to the north or south of the centre of the hill, or nearly so. In observations taken in this manner, the zenith distances of the stars, or the apparent latitude of the station, will be found as they are affected by the attraction of the hill. If then we could by any means know what the zenith distances of the same stars, or what the latitude of the place would have been, if the hill had been away, we should be able to decide on the effect of attraction. This will be found, by repeating the observations of the stars at the east or west end of the hill, where the attraction of the hill, acting in the direction of the prime vertical, has no effect on the plumb-line in the direction of the meridian, nor consequently on the apparent zenith distances of the stars; the differences of the

zenith distances of the stars taken on the north or south side of the hill, and those observed at the east or west end of it, after allowing for the difference of latitude answering to the distance of the parallels of latitude passing through the two stations, will show the quantity of the attraction at the north or south station. But the experiment may be made to more advantage on a hill like Schehallien, which is steep both on the north and south sides, by making the two observations of the stars on both sides; for the plumb-line being attracted contrary ways at the two stations, the apparent zenith distances of stars will be affected contrary ways; those which were increased at the one station being diminished at the other, and consequently their difference will be affected by the sum of the two contrary attractions of the hill. On the south side of the hill, the plumb-line being carried northward at its lower extremity, will occasion the apparent zenith, which is in the direction of the plumb-line, continued backwards, to be carried southward, and consequently to approach the equator; and, therefore, the latitude of the place will appear too small by the quantity of the attraction; the distance of the equator from the zenith being equal to the latitude of the place. The contrary happens on the north side of the hill; the lower extremity of the plumb-line, being there carried southward, will occasion the apparent zenith to be carried northward, or from the equator; and the latitude of the place will appear too great by the quantity of the attraction. Thus the less latitude appearing too small by the attraction on the south side, and the greater latitude appearing too great by the attraction on the north side, the difference of the latitudes will appear too great by the sum of the two contrary attractions; if, therefore, there is an attraction of the hill, the difference of latitude by the celestial observations ought to come out greater than what answers to the distance of the two stations measured trigonometrically, according to the length of a degree of latitude in that parallel, and the observed difference of latitude subtracted from the difference of latitude inferred from the terrestrial operations, will give the sum of the two contrary attractions of the hill. To ascertain the distance between the parallels of latitude passing through the two stations on contrary sides of the hill, a base line must be measured in some level spot near the hill, and connected with the two stations by a chain of triangles, the direction of whose sides, with respect to the meridian, should be settled by astronomical observations.

If it be required, as it ought to be, not only to know the attraction of the hill, but also from it the proportion of the density of the

matter of the hill to the mean density of the earth; then a survey must be made of the hill, to ascertain its dimensions and figure, from which a calculation may be made, how much the hill ought to attract, if its density was equal to the mean density of the earth; it is evident, that the proportion of the actual attraction of the hill, to that computed in this manner, will be the proportion of the density of the hill to the mean density of the earth.

Thus there were three principal operations requisite to be formed. 1. To find by celestial observations the apparent difference of latitude between the two stations, chosen on the north and south sides of the hill. 2. To find the distance between the parallels of latitude. 3. To determine the figure and dimensions of the hill . . .

The difference of latitude found by the astronomical observations, comes out greater than the difference of latitude answering to the distance of the parallels, the former being 54″.6, the latter only 42″.94. The difference 11″.6 is to be attributed to the sum of the 2 contrary attractions of the hill.

The attraction of the hill, computed in a rough manner, on supposition of its density being equal to the mean density of the earth, and the force of attraction being inversely as the square of the distances, comes out about double this. Whence it should follow, that the density of the hill is about half the mean density of the earth. But this point cannot be properly settled till the figure and dimensions of the hill have been calculated from the survey, and thence the attraction of the hill, found from the calculation of several parts of it, into which it is to be divided, which will be a work of much time and labour; the result of which, will be communicated at some future opportunity.

Having thus come to a happy end of this experiment, we may now consider several consequences flowing from it, tending to illustrate some important questions in natural philosophy.

1. It appears from this experiment, that the mountain Schehallien exerts a sensible attraction; therefore, from the rules of philosophizing, we are to conclude, that every mountain, and indeed every particle of the earth, is endued with the same property, in proportion to its quantity of matter.

2. The law of the variation of this force, in the inverse ratio of the squares of the distances, as laid down by Sir Isaac Newton, is also confirmed by this experiment. For, if the force of attraction of the hill has been only to that of the earth, as the matter in the hill to that of the earth, and had not been greatly increased by the

near approach to its centre, the attraction must have been wholly insensible. But now, by only supposing the mean density of the earth to be double that of the hill, which seems very probable from other considerations, the attraction of the hill will be reconciled to the general law of the variation of attraction in the inverse duplicate ratio of the distances, as deduced by Sir Isaac Newton from the comparison of the motion of the heavenly bodies with the force of gravity at the surface of the earth; and the analogy of nature will be preserved.

3. We may now, therefore, be allowed to admit this law; and to acknowledge, that the mean density of the earth is at least double of that at the surface, and consequently, that the density of the internal parts of the earth is much greater than near the surface. Hence also, the whole quantity of matter in the earth will be at least as great again as if it had been all composed of matter of the same density with that at the surface; or will be about 4 or 5 times as great as if it were all composed of water. The idea thus afforded us, from this experiment, of the great density of the internal parts of the earth, is totally contrary to the hypothesis of some naturalists, who suppose the earth to be only a great hollow shell of matter; supporting itself from the property of an arch, with an immense vacuity in the midst of it. But were that the case, the attraction of mountains, and even smaller inequalities in the earth's surface, would be very great, contrary to experiment, and would affect the measures of the degrees of the meridian much more than we find they do; and the variation of gravity in different latitudes, in going from the equator to the poles, as found by pendulums, would not be near so regular as it has been found by experiment to be.

4. The density of the superficial parts of the earth, being, however, sufficient to produce sensible deflections in the plumb-lines of astronomical instruments, will thus cause apparent inequalities in the mensurations of degrees in the meridian; and, therefore, it becomes a matter of great importance to choose those places for measuring degrees, where the irregular attractions of the elevated parts may be small, or in some measure compensate one another; or else it will be necessary to make allowance for their effects, which cannot but be a work of great difficulty, and perhaps liable to great uncertainty.

After all, it is to be wished, that other experiments of the like kinds with this were made in various places, attended with different

circumstances. We seldom acquire full satisfaction from a single experiment on any subject. Some may doubt, whether the density of the matter near the surface of the earth may not be subject to considerable variation; though perhaps, taking large masses together, the density may be more uniform than is commonly imagined, except in hills that have been volcanos. The mountain Schehallien, however, bears not any appearance of having ever been in that state; it being extremely solid and dense, and seemingly composed of an entire rock. New observations on the attraction of other hills, would tend to procure us satisfaction in these points.

SIR WILLIAM HERSCHEL[1]

The Discovery of Uranus

(From "Collected Scientific Papers," Vol. 1, 1912.)

Account of a Comet.—On Tuesday the 13th of March [1781], between ten and eleven in the evening, while I was examining the small stars in the neighbourhood of H Geminorum, I perceived one that appeared visibly larger than the rest; being struck with its uncommon magnitude, I compared it to H Geminorum and the small star in the quartile between Auriga and Gemini, and finding it so much larger than either of them, suspected it to be a comet.

I was then engaged in a series of observations on the parallax of the fixed stars, which I hope soon to have the honour of laying before the Royal Society; and those observations requiring very high powers, I had ready at hand the several magnifiers of 227, 460, 932, 1536, 2010, &c. all which I have successfully used upon that occasion. The power I had on when I first saw the comet was 227. From experience I knew that the diameters of the fixed stars are not proportionally magnified with higher powers, as the planets are; therefore, I now put on the powers of 460 and 932, and found the diameter of the comet increased in proportion to the power, as it ought to be, on a supposition of its not being a fixed star, while the diameters of the stars to which I compared it were not increased in the same ratio. Moreover, the comet being magnified much beyond what its light would admit of, appeared hazy and ill-defined with these great powers, while the stars pre-

[1] Frederick William Herschel (1738–1822), English astronomer, discovered Uranus, the first planet found since prehistoric times. He developed the art of making large reflecting telescopes. A new field in astronomy was opened by his attempt to gain a knowledge of the construction of the heavens through collecting numerical data on stars and nebulæ. Among his contributions were a new method for estimating stellar distribution, and the resulting "grindstone" theory of the Milky Way; the making of catalogues of new nebulæ, nebulosity, and clusters; the first great advance in the discovery and interpretation of double stars; and the discovery of the motion of the sun towards Hercules. The excerpted selections here given were published in the Philosophical Transactions, 1781, 1783, and 1785.

served that lustre and distinctness which from many thousand observations I knew they would retain. The sequel has shewn that my surmises were well founded, this proving to be the Comet we have lately observed . . .

Miscellaneous Observations and Remarks.—March 19. The Comet's apparent motion is at present 2¼ seconds *per* hour. It moves according to the order of the signs, and its orbit declines but very little from the ecliptic.

March 25. The apparent motion of the Comet is accelerating, and its apparent diameter seems to be increasing.

March 28. The diameter is certainly increased, from which we may conclude that the Comet approaches to us . . .

April 6. With a magnifying power of 278 times the Comet appeared perfectly sharp upon the edges, and extremely well defined, without the least appearance of any beard or tail . . .

On the Name of the New Planet.—Sir,—By the observations of the most eminent Astronomers in Europe it appears, that the new star, which I had the honour of pointing out to them in March, 1781, is a Primary Planet of our Solar System. A body so nearly related to us by its similar condition and situation, in the unbounded expanse of the starry heavens, must often be the subject of the conversation, not only of astronomers, but of every lover of science in general. This consideration then makes it necessary to give it a name, whereby it may be distinguished from the rest of the planets and fixed stars.

In the fabulous ages of ancient times the appellations of Mercury, Venus, Mars, Jupiter, and Saturn, were given to the Planets, as being the names of their principal heroes and divinities. In the present more philosophical æra, it would hardly be allowable to have recourse to the same method, and call on Juno, Pallas, Apollo, or Minerva, for a name to our new heavenly body. The first consideration in any particular event, or remarkable incident, seems to be its chronology: if in any future age it should be asked, *when* this last-found Planet was discovered? It would be a very satisfactory answer to say, "In the Reign of King George the Third." As a philosopher then, the name of Georgium Sidus presents itself to me, as an appellation which will conveniently convey the information of the time and country where and when it was brought to view. But as a subject of the best of Kings, who is the liberal protector of every art and science;—as a native of

the country from whence this Illustrious Family was called to the British throne;—as a member of that Society, which flourishes by the distinguished liberality of its Royal Patron;—and, last of all, as a person now more immediately under the protection of this excellent Monarch, and owing everything to His unlimited bounty;—I cannot but wish to take this opportunity of expressing my sense of gratitude, by giving the name Georgium Sidus,

Georgium Sidus,
—jam nunc assuesce vocari. VIRG. GEORG.

to a star, which (with respect to us) first began to shine under His auspicious reign.

By addressing this letter to you, SIR, as President of the Royal Society, I take the most effectual method of communicating that name to the Literati of Europe, which I hope they will receive with pleasure.

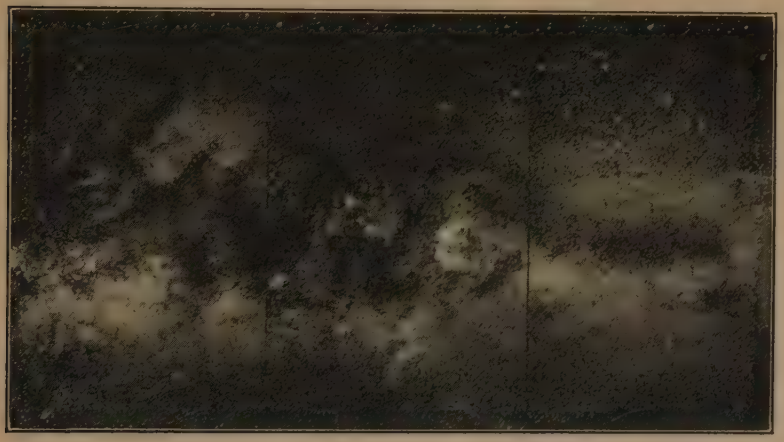
ON THE CONSTRUCTION OF THE HEAVENS

The subject of the Construction of the Heavens, on which I have so lately ventured to deliver my thoughts to this Society, is of so extensive and important a nature, that we cannot exert too much attention in our endeavours to throw all possible light upon it; I shall, therefore, now attempt to pursue the delineations of which a faint outline was begun in my former paper.

By continuing to observe the heavens with my last constructed, and since that time much improved instrument, I am now enabled to bring more confirmation to several parts that were before but weakly supported, and also to offer a few still further extended hints, such as they present themselves to my present view. But first let me mention that, if we would hope to make any progress in an investigation of this delicate nature, we ought to avoid two opposite extremes, of which I can hardly say which is the most dangerous. If we indulge a fanciful imagination and build worlds of our own, we must not wonder at our going wide from the path of truth and nature; but these will vanish like the Cartesian vortices, that soon gave way when better theories were offered. On the other hand, if we add observation to observation, without attempting to draw not only certain conclusions, but also conjectural views from them, we offend against the very end for which

only observations ought to be made. I will endeavour to keep a proper medium; but if I should deviate from that, I could wish not to fall into the latter error.

That the milky way is a most extensive stratum of stars of various sizes admits no longer of the least doubt; and that our sun is actually one of the heavenly bodies belonging to it is as evident. I have now viewed and gaged this shining zone in almost every direction, and find it composed of stars whose number, by the account of these gages, constantly increases and decreases in pro-



Southern Milky Way from Centaurus to Scutum. The center of the galaxy and the richest part of the galactic stream lie in this region partly obscured by dark nebulosities. From photographs made by Bailey with a Harvard telescope in South Africa.

portion to its apparent brightness to the naked eye. But in order to develop the ideas of the universe, that have been suggested by my late observations, it will be best to take the subject from a point of view at a considerable distance both of space and of time.

Theoretical View.—Let us then suppose numberless stars of various sizes, scattered over an indefinite portion of space in such a manner as to be almost equally distributed throughout the whole. The laws of attraction, which no doubt extend to the remotest regions of the fixed stars, will operate in such a manner as most probably to produce the following remarkable effects.

Formation of Nebulæ.—Form I. In the first place, since we have supposed the stars to be of various sizes, it will frequently happen that a star, being considerably larger than its neighbouring

ones, will attract them more than they will be attracted by others that are immediately around them; by which means they will be, in time, as it were, condensed about a center; or, in other words, form themselves into a cluster of stars of almost a globular figure, more or less regularly so, according to the size and original distance of the surrounding stars. The perturbations of these mutual attractions must undoubtedly be very intricate, as we may easily comprehend by considering what SIR ISAAC NEWTON says in the first book of his "Principia," in the 38th and following problems; but in order to apply this great author's reasoning of bodies moving in ellipses to such as are here, for a while, supposed to have no other motion than what their mutual gravity has imparted to them, we must suppose the conjugate axes of these ellipses indefinitely diminished, whereby the ellipses will become straight lines.

Form II. The next case, which will also happen almost as frequently as the former, is where a few stars, though not superior in size to the rest, may chance to be rather nearer each other than the surrounding ones; for here also will be formed a prevailing attraction; in the combined center of gravity of them all, which will occasion the neighbouring stars to draw together; not indeed so as to form a regular or globular figure, but, however, in such a manner as to be condensed towards the common center of gravity of the whole irregular cluster. And this construction admits of the utmost variety of shapes, according to the number and situation of the stars which first gave rise to the condensation of the rest.

Form III. From the composition and repeated conjunction of both the foregoing forms, a third may be derived, when many large stars, or combined small ones, are situated in long extended, regular, or crooked rows, hooks, or branches; for they will also draw the surrounding ones, so as to produce figures of condensed stars coarsely similar to the former which gave rise to these condensations.

Form IV. We may, likewise, admit of still more extensive combinations; when, at the same time that a cluster of stars is forming in one part of space, there may be another collecting in a different, but perhaps not far distant quarter, which may occasion a mutual approach towards their common center of gravity.

Form V. In the last place, as a natural consequence of the former cases, there will be formed great cavities or vacancies by the retreat of the stars towards the various centers which attract them;

so that upon the whole there is evidently a field of the greatest variety for the mutual and combined attractions of the heavenly bodies to exert themselves in. I shall, therefore, without extending myself farther upon this subject, proceed to a few considerations, that will naturally occur to every one who may view this subject in the light I have here done.

Objections Considered.—At first sight then it will seem as if a system, such as it has been displayed in the foregoing paragraphs, would evidently tend to a general destruction, by the shock of one star's falling upon another. It would here be a sufficient answer to say, that if observation should prove this really to be the system of the universe, there is no doubt but that the great Author of it has amply provided for the preservation of the whole, though it should not appear to us in what manner this is effected. But I shall moreover point out several circumstances that do manifestly tend to a general preservational; as, in the first place, the indefinite extent of the sidereal heavens, which must produce a balance that will effectually secure all the great parts of the whole from approaching to each other. There remains then only to see how the particular stars belonging to separate clusters will be preserved from rushing on to their centers of attraction. And here I must observe, that though I have before, by way of rendering the case more simple, considered the stars as being originally at rest, I intended not to exclude projectile forces; and the admission of them will prove such a barrier against the seeming destructive power of attraction as to secure from it all the stars belonging to a cluster, if not for ever, at least for millions of ages. Besides, we ought perhaps to look upon such clusters, and the destruction of now and then a star, in some thousands of ages, as perhaps the very means by which the whole is preserved and renewed. These clusters may be the *Laboratories* of the universe, if I may so express myself, wherein the most salutary remedies for the decay of the whole are prepared.

Optical Appearances.—From this theoretical view of the heavens, which has been taken, as we observed, from a point not less distant in time than in space, we will now retreat to our own retired station, in one of the planets attending a star in its great combination with numberless others; and in order to investigate what will be the appearances from this contracted situation, let us begin with the naked eye. The stars of the first magnitude being in all probability the nearest, will furnish us with a step to begin our scale; setting

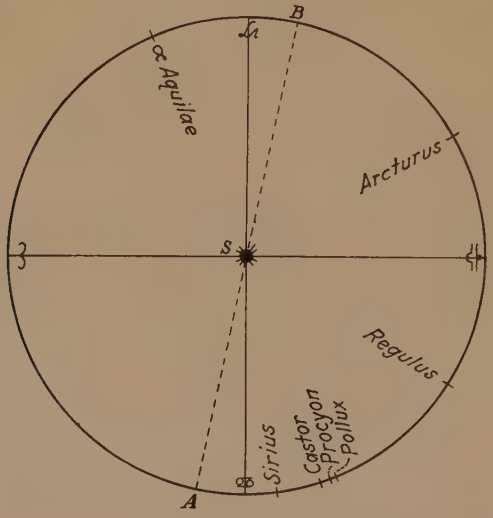
off, therefore, with the distance of Sirius or Arcturus, for instance, as unity, we will at present suppose, that those of the second magnitude are at double, and those of the third at treble the distance, and so forth. It is not necessary critically to examine what quantity of light or magnitude of a star entitles it to be estimated of such or such a proportional distance, as the common coarse estimation will answer our present purpose as well; taking it then for granted, that a star of the seventh magnitude is about seven times as far as one of the first, it follows, that an observer, who is inclosed in a globular cluster of stars, and not far from the center, will never be able, with the naked eye, to see to the end of it: for, since, according to the above estimations, he can only extend his view to about seven times the distance of Sirius, it cannot be expected that his eyes should reach the borders of a cluster which has perhaps not less than fifty stars in depth every where around him. The whole universe, therefore, to him will be comprised in a set of constellations, richly ornamented with scattered stars of all sizes. Or if the united brightness of a neighbouring cluster of stars should, in a remarkable clear night, reach his sight, it will put on the appearance of a small, faint, whitish, nebulous cloud, not to be perceived without the greatest attention. To pass by other situations, let him be placed in a much extended stratum, or branching cluster of millions of stars, such as may fall under the III^{d} form of nebulæ considered in a foregoing paragraph. Here also the heavens will not only be richly scattered over with brilliant constellations, but a shining zone or milky way will be perceived to surround the whole sphere of the heavens, owing to the combined light of those stars which are too small, that is, too remote to be seen. Our observer's sight will be so confined, that he will imagine this single collection of stars, of which he does not even perceive the thousandth part, to be the whole contents of the heavens. Allowing him now the use of a common telescope, be begins to suspect that all the milkiness of the bright path which surrounds the sphere may be owing to stars. He perceives a few clusters of them in various parts of the heavens, and finds also that there are a kind of nebulous patches; but still his views are not extended so far as to reach to the end of the stratum in which he is situated, so that he looks upon these patches as belonging to that system which to him seems to comprehend every celestial object. He now increases his power of vision, and, applying himself to a close observation, finds that the milky way is indeed no other than a

collection of very small stars. He perceives that those objects which had been called nebulæ are evidently nothing but clusters of stars. He finds their number increase upon him, and when he resolves one nebula into stars he discovers ten new ones which he cannot resolve. He then forms the idea of immense strata of fixed stars, of clusters of stars and of nebulæ; till, going on with such interesting observations, he now perceives that all these appearances must naturally arise from the confined situation in which we are placed. *Confined* it may justly be called, though in no less a space than what before appeared to be the whole region of the fixed stars; but which now has assumed the shape of a crookedly branching nebula; not, indeed, one of the least, but perhaps very far from being the most considerable of those numberless clusters that enter into the construction of the heavens.

On the Sun's Motion in Space

Astronomers have already observed what they call a proper motion in several of the fixed stars, and the same may be supposed of them all. We ought, therefore, to resolve that which is common to all the stars, which are found to have what has been called a proper motion, into a single real motion of the solar system, as far as that will answer the known facts, and only to attribute to the proper motion of each particular star the deviations from the general law the stars seem to follow in those movements. By Dr. *Maskelyne's* account of the proper motion of some principal stars, we find that Sirius, Castor, Procyon, Pollux, Regulus, Arcturus, and α Aquilæ, appear to have respectively the following proper motions in right ascension: $-0''.63$; $-0''.28$; $-0''.80$; $-0''.93$; $-0''.41$; $-1''.40$; and $+0''.57$; and two of them, Sirius and Arcturus, in declination, *viz.* $1''.20$ and $2''.01$, both southward. Let the figure represent an equatorial zone, with the above mentioned stars referred to it, according to their respective right ascensions, having the solar system in its center. Assume the direction AB from a point somewhere not far from the 77th degree of right ascension to its opposite 257th degree, and suppose the sun to move in that direction from S towards B; then will that one motion answer that of all the stars together: for if the supposition be true, Arcturus, Regulus, Pollux, Procyon, Castro, and Sirius, should appear to decrease in right ascension, while α Aquilæ, on

the contrary, should appear to increase. Moreover, suppose the sun to ascend at the same time in the same direction towards some point in the northern hemisphere, for instance, towards the constellation of Hercules; then will also the observed change of declination of Sirius and Arcturus be resolved into the single motion of the solar system . . . But lest I should be censured for admitting so new and capital a motion upon too slight a foundation, I must observe, that the concurrence of those seven principal stars cannot but give some value to an hypothesis that will simplify the celestial motions in general. We know that the sun, at the distance of a fixed star, would appear like one of them; and from analogy we



conclude the stars to be suns. Now, since the apparent motions of these seven stars may be accounted for, either by supposing them to move just in the manner they appear to do, or else by supposing the sun alone to have a motion in a direction, somehow not far from that which I have assigned to it, I think we are no more authorized to suppose the sun at rest than we should be to deny the diurnal motion of the earth, except in this respect, that the proofs of the latter are very numerous, whereas the former rests only on a few though capital testimonies. But to proceed: I have only mentioned the motions of those seven principal stars, as being the most noticed and best ascertained of all; I will now adduce a further confirmation of the same from other stars.

M. *de la Lande* gives us the following table of the proper motion of 12 stars, both in right ascension and declination, in 50 years. . . . They are all in the northern hemisphere, except Sirius, which must be supposed to be viewed in the concave part of the opposite half of the globe, while the rest are drawn on the convex surface. Regulus being added to that number, and Castor being double, we have 14 stars. Every star's motion, except Regulus, is assigned in declination as well as in right ascension, so that we have no less than 27 motions given to account for. Now, by assuming a point somewhere near λ Herculis, and supposing the sun to have a proper motion towards that part of the heaven, we shall satisfy 22 of these motions. For β Cygni, α Aquilæ, ε Cygni, γ Piscium, γ Arietis, and Aldebaran, ought, upon the supposed motion of the sun, to have an apparent progression, according to the hour circle XVIII, XIX, XX, &c. or to increase in right ascension, while Arcturus, Regulus, the two stars of α Geminorum, Pollux, Procyon, Sirius, and γ Geminorum, should apparently go back in the order XVI, XV, XIV, &c. of the hour circle, so as to decrease in right ascension; but according to M. *de la Lande's* table, excepting β Cygni and γ Arietis, all these motions really take place. With regard to the change of declination, we see that every star in the table should go towards the south, and here we find but three exceptions in β and ε Cygni, and γ Piscium; so that upon the whole we have but five deviations out of 27 known motions which this hypothesis will not account for.

Etoiles	Chang. d'arc droite	Chang. de declinasion
Arcturus	−1′11″	−1′55″
Sirius	− 37	− 52
β Cygni	− 3	+ 49
Procyon	− 33	− 47
ε Cygni	+ 20	+ 34
γ Arietis	− 14	− 29
γ Gemin	− 8	− 24
Aldébaran	+ 3	− 18
β Gemin	− 48	− 16
γ Piscium	+ 53	+ 7
α Aquilæ	+ 32	− 4
α Gemin	− 24	− 1

And these exceptions must be resolved into the real proper motion of the stars.

There are also some very striking circumstances in the quantities of these motions that deserve our notice. First, Arcturus and Sirius being the largest of the stars, and, therefore, probably the nearest, ought to have the most apparent motion, both in right ascension and declination, which is agreeable to observation, as we find by the table. Next, in regard to the right ascension only, Arcturus being better situated to shew its motion, by theorem 2, ought to have it much larger, which we find it has. Aldebaran, both badly situated and considerably smaller than the two former, by the same theorem ought to shew but little motion. Procyon, better situated than Sirius, though not quite so large, should have almost as much motion; for by the third theorem, on supposing it farther off because it appears smaller, the effect of the sun's motion will be lessened upon it; whereas, on the other hand, by the second theorem, its better situation will partly compensate for its greater distance. This again is conformable to the table. ϵ Cygni very favourably situated, though but a small star, should shew it considerably as well as α Aquilæ; whereas β Cygni should have but little motion; and λ Piscium, best situated of all, should have a great increase of right ascension, and these deductions also agree with the table.

In the last place, a very striking agreement with the hypothesis is displayed in Castor and Pollux. They are both pretty well situated, and we accordingly find that Pollux, for the size of the star, shews as much motion in right ascension as we could expect; but it is remarkable, and seemingly contrary to our hypothesis, that Castor, equally well placed, shews by the table no more than one-half of the motion of Pollux. Now, if we recollect that the former is a double star, consisting of two stars not much different in size, we can allow but about half the light to each of them, which affords a strong presumption of their being at a greater distance, and, therefore, their partial systematical parallax, by the third theorem, ought to be so much less than that of Pollux; which agrees wonderfully with observation. Not to mention the great difficulty in which we should be involved, were we to suppose the motion of Castor to be really in the star: for how extraordinary must appear the concurrence, that two stars, namely those that make up this apparently single star, should both have a proper motion so exactly alike, that in all our observations hitherto, they have not been

found to disagree a single second, either in right ascension or declination, for fifty years together. Does not this seem strongly to point out the common cause, the motion of the solar system?

With respect to the change of declination I would observe, that the point of λ Herculis,[1] . . . is not perhaps the best selected. A somewhat more northern situation may agree better with the changes of declination of Arcturus and Sirius, which capital stars may perhaps be the most proper to lead us in this hypothesis.

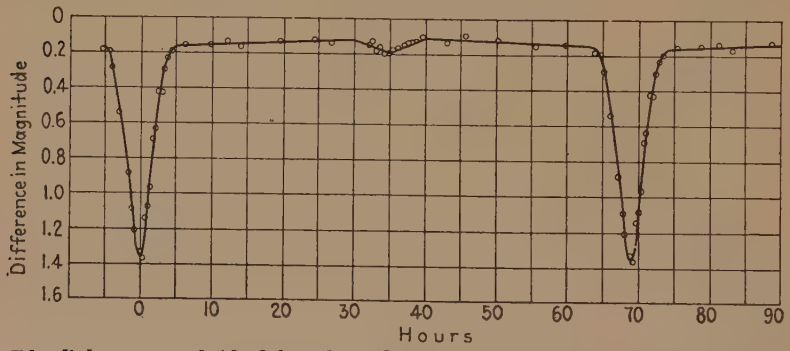
[1 The best modern value of the direction of the sun's motion places the solar apex only seven degrees northwest of λ Herculis.]

GOODRICKE[1]

The Interpretation of the Variability of Algol

(From "Philosophical Transactions," *Abridged*, 1783.)

The following observations, lately made, exhibit a regular and periodical variation in the star Algol or β Persei, of a nature hitherto, I believe, unnoticed. The first time I saw it vary was Nov. 12, 1782, between 8 and 9 o'clock at night, when it appeared of about the 4th magnitude; but the next day it was of the 2d magnitude, which is its usual appearance. On Dec. 28, I per-



The light-curve of Algol based on Stebbins' observations with a selenium photometer, in *Astrophysical Journal*, 1910.

ceived it vary again thus; at $5\frac{1}{2}^{h}$ in the evening, it was about the 4th magnitude, as on the 12th of Nov. but at $8\frac{1}{2}^{h}$ I was much surprised to find it so quickly increased as to appear of the 2d magnitude. The usual and greatest magnitude of Algol is this; of the 2d magnitude, much less bright than α Persei, and not so much as γ Andromedæ; brighter than α Cassiopeæ and β Arietis, and nearly the same, if not rather brighter, than α Pegasi and β Cassiopeæ; rather less bright than γ Cassiopeæ, and much brighter than ϵ Persei and β Trianguli . . .

[1] John Goodricke (1764–1786), of York, England, discovered the period and nature of the variation of Algol. He also discovered the variability of β Lyræ and δ Cephei, the type stars of two important classes of variables.

From a comparison of all the particulars in the observations it appears, first, that this star changes from the 2d to about the 4th magnitude in nearly 3½ hours, and thence to the 2d magnitude again in the same space of time; so that the whole duration of this singular variation is only about 7 hours. And, 2dly, it appears also, that this variation probably recurs about every 2 days and 21 hours. This last conclusion will be rendered more conspicuous by the following table; the first column of which shows the days, and exact time of the day, when Algol was observed to be very near, or at its least brightness; the 2d column marks the different intervals of time elapsed between the several observations; the 3d exhibits the quotient arising from a division of these intervals by a certain number of revolutions, each of 2 days and 21 hours, which number of revolutions are expressed in the last column.

The day and time when Algol was observed at or near its least brightness		The different intervals between the several observations	The quotients of the divisions of the second column by the fourth	Number of revolutions
1782	Nov. 12^d $8\frac{1}{2}^h$			
	Dec. 28 $5\frac{1}{2}$	45^d 21^h	2^d 20.8^h	16
1783	Jan. 14 $9\frac{1}{4}$	17 $3\frac{3}{4}$	2 20.6	6
	31 $14\frac{1}{2}$	17 5	2 20.8	6
	Feb. 6 8	5 $17\frac{3}{4}$	2 21	2
	23 12 +	17 4	2 20.6	6
	26 $9\frac{1}{2}$	2 $21\frac{1}{2}$	2 21.5	1
	Mar. 21 $8\frac{1}{2}$	22 23	2 20.9	8
	Apr. 10 10 +	20 $1\frac{1}{2}$	2 20.8	7
	13 8	2 22	2 22	1
	May 3 $9\frac{1}{4}$	20 1	2 20.7	7

The results in the 3d column agree so nearly, that there is the greatest probability, not to say certainty, that the singular and quick variation of this star, during the space of 7 hours, as above-mentioned, recurs regularly and periodically about every 2 days and nearly $20\frac{3}{4}$ hours.

Whether this singular phenomenon is always the same; or whether it occurs only some years, and ceases entirely in others (as may be presumed from the account of Montanari and Maraldi); and whether in this case it recurs in regular periods of time or otherwise; are curious objects of investigation, which can only be

determined by a long and regular course of observations for many years. If it were not perhaps too early to hazard even a conjecture on the cause of this variation, I should imagine it could hardly be accounted for otherwise than either by the interposition of a large body revolving around Algol,[1] or some kind of motion of its own, by which part of its body, covered with spots or such like matter, is periodically turned towards the earth.

[1 This correct interpretation of Algol was verified in 1889 through spectroscopic work by Professor H. C. Vogel (1841–1907), the director of the Astrophysical Observatory at Potsdam; the first mathematical treatment of the star's orbit was made by E. C. Pickering in 1880, who based his work on accurate photometric observations of the light changes at the time of eclipse.]

LAPLACE[1]

The Nebular Hypothesis

(From "The System of the World," translated by Rev. Henry H. Harte, Vol. 2, 1830.)

However arbitrary the elements of the system of the planets may be, there exists between them some very remarkable relations, which may throw light on their origin. Considering it with attention, we are astonished to see all the planets move round the Sun from west to east, and nearly in the same plane, all the satellites moving round their respective planets in the same direction, and nearly in the same plane with the planets. Lastly, the Sun, the planets, and those satellites in which a motion of rotation have been observed, turn on their own axes, in the same direction, and nearly in the same plane as their motion of projection.

The satellites exhibit in this respect a remarkable peculiarity. Their motion of rotation is exactly equal to their motion of revolution; so that they always present the same hemisphere to their primary. At least, this has been observed for the Moon, for the four satellites of Jupiter, and for the last satellite of Saturn, the only satellites whose rotation has been hitherto recognized.

Phenomena so extraordinary, are not the effect of irregular causes. By subjecting their probability to computation, it is found that there is more than two thousand to one against the hypothesis that they are the effect of chance, which is a probability much greater than that on which most of the events of history, respecting which there does not exist a doubt, depends. We ought, therefore, to be assured with the same confidence, that a primitive cause has directed the planetary motions.

[1] Pierre Simon Laplace (1749–1827), French mathematician, author of two epoch-marking astronomical works, the "Mécanique Céleste" and the "Exposition du Système du Monde," 1796; the former is a survey of the work done on gravitational astronomy since the time of Newton, the latter, an elegant popular treatise on astronomy. The famous Nebular Hypothesis, which has been generally replaced in recent times by the tidal evolution theory of the origin of the planetary system, was a valuable and powerful stimulant to scientific thought throughout the nineteenth century.

Another phenomenon of the solar system, equally remarkable, is the small eccentricity of the orbits of the planets and their satellites, while those of comets are very much extended. The orbits of this system present no intermediate shades between a great and small eccentricity. We are here again compelled to acknowledge the effect of a regular cause; chance alone could not have given a form nearly circular to the orbits of all the planets. It is, therefore, necessary that the cause which determined the motions of these bodies, rendered them also nearly circular. This cause then must also have influenced the great eccentricity of the orbits of comets, and their motion in every direction; for, considering the orbits of retrograde comets, as being inclined more than one hundred degrees to the ecliptic, we find that the mean inclination of the orbits of all the observed comets, approaches near to one hundred degrees, which would be the case if the bodies had been projected at random.

What is this primitive cause? In the concluding note of this work I will suggest an hypothesis which appears to me to result with a great degree of probability, from the preceding phenomena, which, however, I present with that diffidence, which ought always to attach to whatever is not the result of observation and computation.

Whatever be the true cause, it is certain that the elements of the planetary system are so arranged as to enjoy the greatest possible stability, unless it is deranged by the intervention of foreign causes. From the sole circumstance that the motions of the planets and satellites are performed in orbits nearly circular, in the same direction, and in planes which are inconsiderably inclined to each other, the system will always oscillate about a mean state, from which it will deviate but by very small quantities. The mean motions of rotation and of revolution of these different bodies are uniform, and their mean distances from the foci of the principal forces which actuate them are constant; all the secular inequalities are periodic . . .

Note VII, and Last

From the preceding chapter it appears, that we have the five following phenomena to assist us in investigating the cause of the primitive motions of the planetary system. The motions of the planets in the same direction, and very nearly in the same plane;

the motions of the satellites in the same direction as those of the planets; the motions of rotation of these different bodies and also of the Sun, in the same direction as their motions of projection, and in planes very little inclined to each other; the small eccentricity of the orbits of the planets and satellites; finally, the great eccentricity of the orbits of the comets, their inclinations being at the same time entirely indeterminate.

Buffon is the only individual that I know of, who, since the discovery of the true system of the world, endeavoured to investigate the origin of the planets and satellites. He supposed that a comet, by impinging on the Sun, carried away a torrent of matter, which was reunited far off, into globes of different magnitudes, and at different distances from this star. These globes, when they cool and become hardened, are the planets and their satellites. This hypothesis satisfied the first of the five preceding phenomena; for it is evident that all bodies thus formed should move very nearly in the plane which passes through the centre of the Sun, and through the direction of the torrent of matter which has produced them: but the four remaining phenomena appear to me inexplicable on this supposition. Indeed the absolute motion of the molecules of a planet ought to be in the same direction as the motion of its centre of gravity; but it by no means follows from this, that the motion of rotation of a planet should be also in the same direction. Thus the Earth may revolve from east to west, and yet the absolute motion of each of its molecules may be directed from west to east. This observation applies also to the revolution of the satellites, of which the direction, in the same hypothesis, is not necessarily the same as that of the motion of projection of the planets.

The small eccentricity of the planetary orbits is a phenomenon, not only difficult to explain on this hypothesis, but altogether inconsistent with it. We know from the theory of central forces, that if a body which moves in a re-entrant orbit about the Sun, passes very near the body of the Sun, it will return constantly to it, at the end of each revolution. Hence it follows that if the planets were originally detached from the Sun, they would touch it, at each return to this star; and their orbits, instead of being nearly circular, would be very eccentric. Indeed it must be admitted that a torrent of matter detached from the Sun, cannot be compared to a globe which just skims by its surface: from the impulsions which the parts of this torrent receive from each other,

combined with their mutual attraction, they may, by changing the direction of their motions, increase the distances of their perihelions from the Sun. But their orbits should be extremely eccentric, or at least all the orbits would not be circular, except by the most extraordinary chance. Finally, no reason can be assigned on the hypothesis of Buffon, why the orbits of more than one hundred comets, which have been already observed, should be all very eccentric. This hypothesis, therefore, is far from satisfying the preceding phenomena. Let us consider whether we can assign the true cause.

Whatever may be its nature, since it has produced or influenced the direction of the planetary motions, it must have embraced them all within the sphere of its action; and considering the immense distance which intervenes between them, nothing could have effected this but a fluid of almost indefinite extent. In order to have impressed on them all a motion circular and in the same direction about the Sun, this fluid must environ this star, like an atmosphere. From a consideration of the planetary motions, we are, therefore, brought to the conclusion, that in consequence of an excessive heat, the solar atmosphere originally extended beyond the orbits of all the planets, and that it has successively contracted itself within its present limits.

In the primitive state in which we have supposed the Sun to be, it resembles those substances which are termed nebulæ, which, when seen through telescopes, appear to be composed of a nucleus, more or less brilliant, surrounded by a nebulosity, which, by condensing on its surface, transforms it into a star. If all the stars are conceived to be similarly formed, we can suppose their anterior state of nebulosity to be preceded by other states, in which the nebulous matter was more or less diffuse, the nucleus being at the same time more or less brilliant. By going back in this manner, we shall arrive at a state of nebulosity so diffuse, that its existence can with difficulty be conceived.

For a considerable time back, the particular arrangement of some stars visible to the naked eye, has engaged the attention of philosophers. Mitchel remarked long since how extremely improbable it was that the stars composing the constellation called the Pleiades, for example, should be confined within the narrow space which contains them, by the sole chance of hazard; from which he inferred that this group of stars, and the similar groups which the heavens present to us, are the effects of a primitive cause, or of a

primitive law of nature. These groups are a general result of the condensation of nebulæ of several nuclei; for it is evident that the nebulous matter being perpetually attracted by these different nuclei, ought at length to form a group of stars, like to that of the Pleiades. The condensation of nebulæ consisting of two nuclei, will in like manner form stars very near to each other, revolving the one about the other like to the double stars, whose respective motions have been already recognized.

But in what manner has the solar atmosphere determined the motions of rotation and revolution of the planets and satellites? If these bodies had penetrated deeply into this atmosphere, its resistance would cause them to fall on the Sun. We may, therefore, suppose that the planets were formed at its successive limits, by the condensation of zones of vapours, which it must, while it was cooling, have abandoned in the plane of its equator.

Let us resume the results which we have given in the tenth chapter of the preceding book. The Sun's atmosphere cannot extend indefinitely; its limit is the point where the centrifugal force arising from the motion of rotation balances the gravity; but according as the cooling contracts the atmosphere, and condenses the molecules which are near to it, on the surface of the star, the motion of rotation increases; for in virtue of the principle of areas, the sum of the areas described by the radius vector of each particle of the Sun and of its atmosphere, and projected on the plane of its equator, is always the same. Consequently, the rotation ought to be quicker, when these particles approach to the centre of the Sun. The centrifugal force arising from this motion becoming thus greater, the point where the gravity is equal to it, is nearer to the centre of the Sun. Supposing, therefore, what is natural to admit, that the atmosphere extended at any epoch as far as this limit, it ought, according as it cooled, to abandon the molecules, which are situated at this limit, and at the successive limits produced by the increased rotation of the Sun. These particles, after being abandoned, have continued to circulate about this star, because their centrifugal force was balanced by their gravity. But as this equality does not obtain for those molecules of the atmosphere which are situated on the parallels to the Sun's equator, these have come nearer by their gravity to the atmosphere according as it condensed, and they have not ceased to belong to it, inasmuch as by this motion, they have approached to the plane of this equator.

Let us now consider the zones of vapours, which have been successively abandoned. These zones ought, according to all probability, to form by their condensation, and by the mutual attraction of their particles, several concentrical rings of vapours circulating about the Sun. The mutual friction of the molecules of each ring ought to accelerate some and retard others, until they all had acquired the same angular motion. Consequently, the real velocities of the molecules which are farther from the Sun, ought to be greatest. The following cause ought, likewise, to contribute to this difference of velocities: The most distant particles of the Sun, which, by the effects of cooling and of condensation, have collected so as to constitute the superior part of the ring, have always described areas proportional to the times, because the central force by which they are actuated has been constantly directed to this star; but this constancy of areas requires an increase of velocity, according as they approach more to each other. It appears that the same cause ought to diminish the velocity of the particles, which, situated near the ring, constitute its inferior part.

If all the particles of a ring of vapours continued to condense without separating, they would at length constitute a solid or a liquid ring. But the regularity which this formation requires in all the parts of the ring, and in their cooling, ought to make this phenomenon very rare. Thus the solar system presents but one example of it; that of the rings of Saturn. Almost always each ring of vapours ought to be divided into several masses, which, being moved with velocities which differ little from each other, should continue to revolve at the same distance about the Sun. These masses should assume a spheroidical form, with a rotatory motion in the direction of that of their revolution, because their inferior particles have a less real velocity than the superior; they have, therefore, constituted so many planets in a state of vapour. But if one of them was sufficiently powerful, to unite successively by its attraction, all the others about its centre, the ring of vapours would be changed into one sole spheroidical mass, circulating about the Sun, with a motion of rotation in the same direction with that of revolution. This last case has been the most common; however, the solar system presents to us the first case, in the four small planets which revolve between Mars and Jupiter, at least unless we suppose with Olbers, that they originally formed one planet only, which was divided by an explosion into several parts, and actuated by different velocities. Now if we trace the changes

which a farther cooling ought to produce in the planets formed of vapours, and of which we have suggested the formation, we shall see to arise in the centre of each of them, a nucleus increasing continually, by the condensation of the atmosphere which environs it. In this state, the planet resembles the Sun in the nebulous state, in which we have first supposed it to be; the cooling should, therefore, produce at the different limits of its atmosphere, phenomena similar to those which have been described, namely, rings and satellites circulating about its centre in the direction of its motion of rotation, and revolving in the same direction on their axes. The regular distribution of the mass of rings of Saturn about its centre and in the plane of its equator, results naturally from this hypothesis, and, without it, is inexplicable. Those rings appear to me to be existing proofs of the primitive extension of the atmosphere of Saturn, and of its successive condensations. Thus the singular phenomena of the small eccentricities of the orbits of the planets and satellites, of the small inclination of these orbits to the solar equator, and of the indentity in the direction of the motions of rotation and revolution of all those bodies with that of the rotation of the Sun, follow from the hypothesis which has been suggested, and render it extremely probable. If the solar system was formed with perfect regularity, the orbits of the bodies which compose it would be circles, of which the planes, as well as those of the various equators and rings, would coincide with the plane of the solar equator. But we may suppose that the innumerable varieties which must necessarily exist in the temperature and density of different parts of these great masses, ought to produce the eccentricities of their orbits, and the deviations of their motions, from the plane of this equator.

In the preceding hypothesis, the comets do not belong to the solar system. If they be considered, as we have done, as small nebulæ, wandering from one solar system to another, and formed by the condensation of the nebulous matter, which is diffused so profusely throughout the universe, we may conceive that when they arrive in that part of space where the attraction of the Sun predominates, it should force them to describe elliptic or hyperbolic orbits. But as their velocities are equally possible in every direction, they must move indifferently in all directions, and at every possible inclination to the ecliptic; which is conformable to observation. Thus the condensation of the nebulous matter, which explains the motions of rotation and revolution of the

planets and satellites in the same direction, and in orbits very little inclined to each other, likewise explains why the motions of the comets deviate from this general law.

The great eccentricity of the orbits of the comets, is also a result of our hypothesis. If those orbits are elliptic, they are very elongated, since their greater axes are at least equal to the radius of the sphere of activity of the Sun. But these orbits may be hyperbolic; and if the axes of these hyperbolæ are not very great with respect to the mean distance of the Sun from the Earth, the motion of the comets which describe them will appear to be sensibly hyperbolic. However, with respect to the hundred comets, of which the elements are known, not one appears to move in a hyperbola; hence the chances which assign a sensible hyperbola, are extremely rare relatively to the contrary chances. The comets are so small, that they only become sensible when their perihelion distance is inconsiderable. Hitherto this distance has not surpassed twice the diameter of the Earth's orbit, and most frequently, it has been less than the radius of this orbit. We may conceive, that in order to approach so near to the Sun, their velocity at the moment of their ingress within its sphere of activity, must have an intensity and direction confined within very narrow limits. If we determine by the analysis of probabilities, the ratio of the chances which, in these limits, assign a sensible hyperbola to the chances which assign an orbit, which may without sensible error be confounded with a parabola, it will be found that there is at least six thousand to unity that a nebula which penetrates within the sphere of the Sun's activity so as to be observed, will either describe a very elongated ellipse, or an hyperbola, which, in consequence of the magnitude of its axis will be as to sense confounded with a parabola in the part of its orbit which is observed. It is not, therefore, surprising that hitherto no hyperbolic motions have been recognised.

The attraction of the planets, and perhaps also the resistance of the ethereal media, ought to change several cometary orbits into ellipses, of which the greater axes are much less than the radius of the sphere of the solar activity. It is probable that such a change was produced in the orbit of the comet of 1759, the greater axis of which was not more than thirty-five times the distance of the Sun from the Earth. A still greater change was produced in the orbits of the comets of 1770 and of 1805.

If any comets have penetrated the atmospheres of the Sun and planets at the moment of their formation, they must have described

spirals, and consequently fallen on these bodies, and in consequence of their fall, caused the planes of the orbits and of the equators of the planets to deviate from the plane of the solar equator.

If in the zones abandoned by the atmosphere of the Sun, there are any molecules too volatile to be united to each other, or to the planets, they ought, in their circulation about this star, to exhibit all the appearances of the zodiacal light, without opposing any sensible resistance to the different bodies of the planetary system, both on account of their great rarity, and also because their motion is very nearly the same as that of the planets which they meet.

An attentive examination of all the circumstances of this system renders our hypothesis still more probable. The primitive fluidity of the planets is clearly indicated by the compression of their figure, conformably to the laws of the mutual attraction of their molecules; it is, moreover, demonstrated by the regular diminution of gravity, as we proceed from the equator to the poles. This state of primitive fluidity to which we are conducted by astronomical phenomena, is also apparent from those which natural history points out. But in order fully to estimate them, we should take into account the immense variety of combinations formed by all the terrestrial substances which were mixed together in a state of vapour, when the depression of their temperature enabled their elements to unite; it is necessary, likewise, to consider the wonderful changes which this depression ought to cause in the interior and at the surface of the earth, in all its productions, in the constitution and pressure of the atmosphere, in the ocean, and in all substances which it held in a state of solution. Finally, we should take into account the sudden changes, such as great volcanic eruptions, which must at different epochs have deranged the regularity of these changes. Geology, thus studied under the point of view which connects it with astronomy, may, with respect to several objects, acquire both precision and certainty.

One of the most remarkable phenomena of the solar system is the rigorous equality which is observed to subsist between the angular motions of rotation and revolution of each satellite. It is infinity to unity that this is not the effect of hazard. The theory of universal gravitation makes infinity to disappear from this improbability, by shewing that it is sufficient for the existence of this phenomenon, that at the commencement these motions did not differ much. Then, the attraction of the planet would establish between them a perfect equality; but at the same time it

has given rise to a periodic oscillation in the axis of the satellite directed to the planet, of which oscillation the extent depends on the primitive difference between these motions. As the observations of Mayer on the libration of the Moon, and those which Bouvard and Nicollet made for the same purpose, at my request, did not enable us to recognize this oscillation; the difference on which it depends must be extremely small, which indicates with every appearance of probability the existence of a particular cause, which has confined this difference within very narrow limits, in which the attraction of the planet might establish between the mean motions of rotation and revolution a rigid equality, which at length terminated by annihilating the oscillation which arose from this equality. Both these effects result from our hypothesis; for we may conceive that the Moon, in a state of vapour, assumed in consequence of the powerful attraction of the earth the form of an elongated spheroid, of which the greater axis would be constantly directed towards this planet, from the facility with which the vapours yield to the slightest force impressed upon them. The terrestrial attraction continuing to act in the same manner, while the Moon is in a state of fluidity, ought at length, by making the two motions of this satellite to approach each other, to cause their difference to fall within the limits, at which their rigorous equality commences to establish itself. Then this attraction should annihilate, by little and little, the oscillation which this equality produced on the greater axis of the spheroid directed towards the earth. It is in this manner that the fluids which cover this planet, have destroyed by their friction and resistance the primitive oscillations of its axis of rotation, which is only now subject to the nutation resulting from the actions of the Sun and Moon. It is easy to be assured that the equality of the motions of rotation and revolution of the satellites ought to oppose the formation of rings and secondary satellites, by the atmospheres of these bodies. Consequently observation has not hitherto indicated the existence of any such. The motions of the three first satellites of Jupiter present a phenomenon still more extraordinary than the preceding; which consists in this, that the mean longitude of the first, minus three times that of the second, plus twice that of the third, is constantly equal to two right angles. There is the ratio of infinity to one, that this equality is not the effect of chance. But we have seen, that in order to produce it, it is sufficient, if at the commencement, the mean motions of these three bodies

approached very near to the relation which renders the mean motion of the first, minus three times that of the second, plus twice that of the third, equal to nothing. Then their mutual attraction rendered this ratio rigorously exact, and it has moreover made the mean longitude of the first minus three times that of the second, plus twice that of the third, equal to a semicircumference. At the same time, it gave rise to a periodic inequality, which depends on the small quantity, by which the mean motions originally deviated from the relation which we have just announced. Notwithstanding all the care Delambre took in his observations, he could not recognise this inequality, which, while it evinces its extreme smallness, also indicates, with a high degree of probability, the existence of a cause which makes it to disappear. In our hypothesis, the satellites of Jupiter, immediately after their formation, did not move in a perfect vacuo; the less condensible molecules of the primitive atmospheres of the Sun and planet would then constitute a rare medium, the resistance of which being different for each of the [bodies], might make the mean motions to approach by degrees to the ratio in question; and when these movements had thus attained the conditions requisite, in order that the mutual attraction of the three satellites might render this relation accurately true, it perpetually diminished the inequality which this relation originated, and eventually rendered it insensible. We cannot better illustrate these effects than by comparing them to the motion of a pendulum, which, actuated by a great velocity, moves in a medium, the resistance of which is inconsiderable. It will first describe a great number of circumferences; but at length its motion of circulation perpetually decreasing, it will be converted into an oscillatory motion, which itself diminishing more and more, by the resistance of the medium, will eventually be totally destroyed, and then the pendulum, having attained a state of repose, will remain at rest for ever.

Mécanique Céleste

(From the preface of Vol. 3, 1802; translated by Nathaniel Bowditch, 1834.)

We have given, in the first part of this work, the general principles of the equilibrium and motion of bodies. The application of these principles to the motions of the heavenly bodies, has conducted us, by geometrical reasoning, without any hypothesis, to the law of universal attraction; the action of gravity, and the

motions of projectiles on the surface of the earth, being particular cases of this law. We have then taken into consideration, a system of bodies subjected to this great law of nature; and have obtained, by a singular analysis, the general expressions of their motions, of their figures, and of the oscillations of the fluids which cover them. From these expressions, we have deduced all the known phenomena of the flow and ebb of the tide; the variations of the degrees, and of the force of gravity at the surface of the earth; the precession of the equinoxes; the libration of the moon; and the figure and rotation of Saturn's Rings. We have also pointed out the cause, why these rings remain, permanently, in the plane of the equator of Saturn. Moreover, we have deduced, from the same theory of gravity, the principal equations of the motions of the planets; particularly those of Jupiter and Saturn, whose great inequalities have a period of above nine hundred years.

The inequalities in the motions of Jupiter and Saturn, presented, at first, to astronomers, nothing but anomalies, whose laws and causes were unknown, and, for a long time, these irregularities appeared to be inconsistent with the theory of gravity; but a more thorough examination has shown, that they can be deduced from it; and now, these motions are one of the most striking proofs of the truth of this theory. We have developed the secular variations of the elements of the planetary system, which do not return to the same state till after the lapse of many centuries.

In the midst of all these changes we have discovered the constancy of the mean motions, and of the mean distances of the bodies of this system; which nature seems to have arranged, at its origin, for an eternal duration, upon the same principles as those which prevail, so admirably, upon the earth, for the preservation of individuals, and for the perpetuity of the species. From the single circumstance, that the motions are all in the same direction, and in planes but little inclined to each other, it follows, that the orbits of the planets and satellites must always be nearly circular, and but little inclined to each other. Thus, the variations of the obliquity of the ecliptic, which are always included within narrow limits, will never produce an eternal spring upon the earth. We have proved that the attraction of the terrestrial spheroid, by incessantly drawing towards its centre the hemisphere of the moon, which is directed towards the earth, transfers to the

rotatory motion of this satellite, the great secular variations of its motion of revolution; and, by this means, keeps always from our view, the other hemisphere. Lastly, we have demonstrated, in the motions of the three first satellites of Jupiter, the following remarkable law, namely, that, in consequence of their mutual attractions, *the mean longitude of the first satellite, seen from the centre of Jupiter, minus three times that of the second satellite, plus twice that of the third satellite, is always exactly equal to two right angles;* so that they cannot all be eclipsed at the same time. It remains now to consider particularly the perturbations of the motions of the planets and comets about the sun; of the moon about the earth; and of the satellites about their primary planets. This is the object of the second part of this work, which is particularly devoted to the improvement of astronomical tables . . .

It is chiefly in the application of *analysis* to the system of the world, that we perceive the power of this wonderful instrument; without which, it would have been impossible to have discovered a mechanism which is so complicated in its effects, while it is so simple in its cause. The mathematician now includes in his formulas, the whole of the planetary system, and its successive variations; he looks back, in imagination, to the several states, which the system has passed through, in the most remote ages; and foretells what time will hereafter make known to observers. He sees this sublime spectacle, whose period includes several millions of years, repeated in a few centuries, in the system of the satellites of Jupiter, by means of the rapidity of their revolutions; which produce remarkable phenomena, similar to those which had been suspected, by astronomers, in the planetary motions; but had not been determined, because they were either too complex, or too slow, for an accurate determination of their laws. The theory of gravity, which, by so many applications, has become a means of discovery, as certain as by observation itself, has made known to him several new inequalities, in the motions of the heavenly bodies, and enabled him to predict the return of the comet of 1759, whose revolutions are rendered very unequal, by the attractions of Jupiter and Saturn. He has been enabled, by this means, to deduce, from observation, as from a rich mine, a great number of important and delicate elements, which, without the aid of analysis, would have been forever hidden from his view: such as the rela-

tive values of the masses of the sun, the planets and satellites, determined by the revolutions of these bodies, and by the development of their periodical and secular inequalities: the velocity of light, and the ellipticity of Jupiter; which are given, by the eclipses of its satellites, with greater accuracy, than by direct observation: the rotation and oblateness of Uranus and Saturn; deduced from the consideration, that the different bodies which revolve about those two planets, are in the same plane, respectively: the parallaxes of the sun and moon: and, also, the figure of the earth, deduced from some lunar inequalities: for, we shall see hereafter, that the moon, by its motion, discloses to modern astronomy, the small ellipticity of the terrestrial spheroid, whose roundness was made known to the first observers by the eclipses of that luminary. Lastly, by a fortunate combination of analysis with observation, that body, which seems to have been given to the earth, to enlighten it, during the night, becomes also the most sure guide of the navigator; who is protected by it from the dangers, to which he was for a long time exposed, by the errors of his *reckoning*. The perfection of the theory, and of the lunar tables, to which he is indebted for this important object, and for that of determining, with accuracy, the position of the places he falls in with, is the fruit of the labors of mathematicians and astronomers, during the last fifty years: it unites all that can give value to a discovery; the importance and usefulness of the object, its various applications, and the merit of the difficulty which is overcome. It is thus, that the most abstract theories, diffused by numerous applications to nature and to the arts, have become inexhaustible sources of comfort and enjoyment, even to those who are wholly ignorant of the nature of these theories.

Probabilities and Natural Philosophy[1]

(From "Philosophical Essay on Probabilities," 6th ed., 1840; translated by F. W. Truscott and F. L. Emory, 1902.)

All events, even those which on account of their insignificance do not seem to follow the great laws of nature, are a result of it just as necessarily as the revolutions of the sun. In ignorance of the ties which unite such events to the entire system of the universe, they have been made to depend upon final causes or upon

[1 1st ed., 1814.]

hazard, according as they occur and are repeated with regularity, or appear without regard to order; but these imaginary causes have gradually receded with the widening bounds of knowledge and disappear entirely before sound philosophy, which sees in them only the expression of our ignorance of the true causes.

Present events are connected with preceding ones by a tie based upon the evident principle that a thing cannot occur without a cause which produces it. This axiom, known by the name of *the principle of sufficient reason*, extends even to actions which are considered indifferent; the freest will is unable without a determinative motive to give them birth; if we assume two positions with exactly similar circumstances and find that the will is active in the one and inactive in the other, we say that its choice is an effect without a cause. It is then, says Leibnitz, the blind chance of the Epicureans. The contrary opinion is an illusion of the mind, which, losing sight of the evasive reasons of the choice of the will in indifferent things, believes that choice is determined of itself and without motives.

We ought then to regard the present state of the universe as the effect of its anterior state and as the cause of the one which is to follow. Given for one instant an intelligence which could comprehend all the forces by which nature is animated and the respective situation of the beings who compose it—an intelligence sufficiently vast to submit these data to analysis—it would embrace in the same formula the movements of the greatest bodies of the universe and those of the lightest atom; for it, nothing would be uncertain and the future, as the past, would be present to its eyes. The human mind offers, in the perfection which it has been able to give to astronomy, a feeble idea of this intelligence. Its discoveries in mechanics and geometry, added to that of universal gravity, have enabled it to comprehend in the same analytical expressions the past and future states of the system of the world. Applying the same method to some other objects of its knowledge, it has succeeded in referring to general laws observed phenomena and in foreseeing those which given circumstances ought to produce. All these efforts in the search for truth tend to lead it back continually to the vast intelligence which we have just mentioned, but from which it will always remain infinitely removed. This tendency, peculiar to the human race, is that which renders it superior to animals; and their progress in this respect distinguishes nations and ages and constitutes their true glory.

Let us recall that formerly, and at no remote epoch, an unusual rain or an extreme drought, a comet having in train a very long tail, the eclipses, the aurora borealis, and in general all the unusual phenomena were regarded as so many signs of celestial wrath. Heaven was invoked in order to avert their baneful influence. No one prayed to have the planets and the sun arrested in their courses; observation had soon made apparent the futility of such prayers. But as these phenomena, occurring and disappearing at long intervals, seemed to oppose the order of nature, it was supposed that Heaven, irritated by the crimes of the earth, had created them to announce its vengeance. Thus the long tail of the comet of 1456 spread terror through Europe, already thrown into consternation by the rapid successes of the Turks, who had just overthrown the Lower Empire. This star after four revolutions has excited among us a very different interest. The knowledge of the laws of the system of the world acquired in the interval had dissipated the fears begotten by the ignorance of the true relationship of man to the universe; and Halley, having recognized the identity of this comet with those of the years 1531, 1607, and 1682, announced its next return for the end of the year 1758 or the beginning of the year 1759. The learned world awaited with impatience this return which was to confirm one of the greatest discoveries that have been made in the sciences, and fulfil the prediction of Seneca when he said, in speaking of the revolutions of those stars which fall from an enormous height: "The day will come when, by study pursued through several ages, the things now concealed will appear with evidence; and posterity will be astonished that truths so clear had escaped us." Clairaut then undertook to submit to analysis the perturbations which the comet had experienced by the action of the two great planets, Jupiter and Saturn; after immense calculations he fixed its next passage at the perihelion toward the beginning of April, 1759, which was actually verified by observation. The regularity which astronomy shows us in the movements of the comets doubtless exists also in all phenomena.

The curve described by a simple molecule of air or vapor is regulated in a manner just as certain as the planetary orbits; the only difference between them is that which comes from our ignorance.

Probability is relative, in part to this ignorance, in part to our knowledge. We know that of three or a greater number of events

a single one ought to occur; but nothing induces us to believe that one of them will occur rather than the others. In this state of indecision it is impossible for us to announce their occurrence with certainty. It is, however, probable that one of these events, chosen at will, will not occur because we see several cases equally possible which exclude its occurrence, while only a single one favors it.

The theory of chance consists in reducing all the events of the same kind to a certain number of cases equally possible, that is to say, to such as we may be equally undecided about in regard to their existence, and in determining the number of cases favorable to the event whose probability is sought. The ratio of this number to that of all the cases possible is the measure of this probability, which is thus simply a fraction whose numerator is the number of favorable cases and whose denominator is the number of all the cases possible.

The preceding notion of probability supposes that, in increasing in the same ratio the number of favorable cases and that of all the cases possible, the probability remains the same. In order to convince ourselves let us take two urns, *A* and *B*, the first containing four white and two black balls, and the second containing only two white balls and one black one. We may imagine the two black balls of the first urn attached by a thread which breaks at the moment when one of them is seized in order to be drawn out, and the four while balls thus forming two similar systems. All the chances which will favor the seizure of one of the balls of the black system will lead to a black ball. If we conceive now that the threads which unite the balls do not break at all, it is clear that the number of possible chances will not change any more than that of the chances favorable to the extraction of the black balls; but two balls will be drawn from the urn at the same time; the probability of drawing a black ball from the urn *A* will then be the same as at first. But then we have obviously the case of urn *B* with the single difference that the three balls of this last urn would be replaced by three systems of two balls invariably connected.

When all the cases are favorable to an event the probability changes to certainty and its expression becomes equal to unity. Upon this condition, certainty and probability are comparable, although there may be an essential difference between the two states of the mind when a truth is rigorously demonstrated to it, or when it still perceives a small source of error.

In things which are only probable the difference of the data, which each man has in regard to them, is one of the principal causes of the diversity of opinions which prevail in regard to the same objects. Let us suppose, for example, that we have three urns, *A*, *B*, *C*, one of which contains only black balls while the two others contain only white balls; a ball is to be drawn from the urn *C* and the probability is demanded that this ball will be black. If we do not know which of the three urns contains black balls only, so that there is no reason to believe that it is *C* rather than *B* or *A*, these three hypotheses will appear equally possible, and since a black ball can be drawn only in the first hypothesis, the probability of drawing is equal to one third. If it is known that the urn *A* contains white balls only, the indecision then extends only to the urns *B* and *C*, and the probability that the ball drawn from the urn *C* will be black in one half. Finally, this probability changes to certainty if we are assured that the urns *A* and *B* contain white balls only.

It is thus that an incident related to a numerous assembly finds various degrees of credence, according to the extent of knowledge of the auditors. If the man who reports it is fully convinced of it and if, by his position and character, he inspires great confidence, his statement, however extraordinary it may be, will have for the auditors who lack information, the same degree of probability as an ordinary statement made by the same man, and they will have entire faith in it. But if some one of them knows that the same incident is rejected by other equally trustworthy men, he will be in doubt and the incident will be discredited by the enlightened auditors, who will reject it whether it be in regard to facts well averred or the immutable laws of nature.

It is to the influence of the opinion of those whom the multitude judges best informed and to whom it has been accustomed to give its confidence in regard to the most important matters of life that the propagation of those errors is due which in times of ignorance have covered the face of the earth. Magic and astrology offer us two great examples. These errors inculcated in infancy, adopted without examination, and having for a basis only universal credence, have maintained themselves during a very long time; but at last the progress of science has destroyed them in the minds of enlightened men, whose opinion consequently has caused them to disappear even among the common people, through the power of imitation and habit which had so generally spread them abroad. This power, the richest resource of the moral world, establishes and

conserves in a whole nation ideas entirely contrary to those which it upholds elsewhere with the same authority. What indulgence ought we not then, to have for opinions different from ours, when this difference often depends only upon the various points of view where circumstances have placed us! Let us enlighten those whom we judge insufficiently instructed; but first, let us examine critically our own opinions and weigh with impartiality their respective probabilities.

The difference of opinions depends, however, upon the manner in which the influence of known data is determined. The theory of probabilities holds to considerations so delicate that it is not surprising that with the same data two persons arrive at different results, especially in very complicated questions . . .

The astronomical tables owe the truly astonishing exactitude which they have attained to the precision of observations and of theories, and to the use of equations of conditions which cause to concur a great number of excellent observations in the correction of the same element. But it remains to determine the probability of the errors that this correction leaves still to be feared; and the method which I have just explained enables us to recognize the probability of these errors. In order to give some interesting applications of it I have profited by the immense work which M. Bouvard has just finished on the movements of Jupiter and Saturn, of which he has formed very precise tables. He has discussed with the greatest care the oppositions and quadratures of these two planets observed by Bradley and by the astronomers who have followed him down to the last years; he has concluded the corrections of the elements of their movement and their masses compared to that of the sun taken as unity. His calculations give him the mass of Saturn equal to the 3512th part of that of the sun. Applying to them my formulæ of probability, I find that it is a bet of 11,000 against one that the error of this result is not $\frac{1}{100}$ of its value, or that which amounts to almost the same—that after a century of new observations added to the preceding ones, and examined in the same manner, the new result will not differ by $\frac{1}{100}$ from that of M. Bouvard. This wise astronomer finds again the mass of Jupiter equal to the 1071th part of the sun; and my method of probability gives a bet of 1,000,000 to one that this result is not $\frac{1}{100}$ in error . . .

The consideration of probabilities can serve to distinguish the small irregularities of the celestial movements enveloped in the

errors of observations, and to repass to the cause of the anomalies observed in these movements.

In comparing all the observations it was Tycho Brahe who recognized the necessity of applying to the moon an equation of time different from that which had been applied to the sun and to the planets. It was similarly the totality of a great number of observations which made Mayer recognize that the coefficient of the inequality of the precession ought to be diminished a little for the moon. But since this diminution, although confirmed and even augmented by Mason, did not appear to result from universal gravitation, the majority of astronomers neglect it in their calculations. Having submitted to the calculation of probabilities a considerable number of lunar observations chosen for this purpose and which M. Bouvard consented to examine at my request, it appeared to me to be indicated with so strong a probability that I believed the cause of it ought to be investigated. I soon saw that it would be only the ellipticity of the terrestrial spheroid, neglected up to that time in the theory of the lunar movement as being able to produce only imperceptible terms. I concluded that these terms became perceptible by the successive integrations of differential equations. I determined then those terms by a particular analysis, and I discovered first the inequality of the lunar movement in latitude which is proportional to the sine of the longitude of the moon, which no astronomer before had suspected. I recognized then by means of this inequality that another exists in the lunar movement in longitude which produces the diminution observed by Mayer in the equation of the precession applicable to the moon. The quantity of this diminution and the coefficient of the preceding inequality in latitude are very appropriate to fix the oblateness of the earth. Having communicated my researches to Mr. Burg, who was occupied at that time in perfecting the tables of the moon by the comparison of all the good observations, I requested him to determine with a particular care these two quantities. By a very remarkable agreement the values which he has found give to the earth the same oblateness, $\frac{1}{305}$, which differs little from the mean derived from the measurements of the degrees of the meridian and the pendulum; but those regarded from the point of view of the influence of the errors of the observations and of the perturbing causes in these measurements, did not appear to me exactly determined by these lunar inequalities.

It was again by the consideration of probabilities that I recognized the cause of the secular equation of the moon. The modern

observations of this [body] compared to the ancient eclipses had indicated to astronomers an acceleration in the lunar movement; but the geometricians, and particularly Lagrange, having vainly sought in the perturbations which this movement experienced the terms upon which this acceleration depends, reject it. An attentive examination of the ancient and modern observations and of the intermediary eclipses observed by the Arabians convinced me that it was indicated with a great probability. I took up again then from this point of view the lunar theory, and I recognized that the secular equation of the moon is due to the action of the sun upon this satellite, combined with the secular variation of the eccentricity of the terrestrial orb; this brought me to the discovery of the secular equations of the movements of the nodes and the perigees of the lunar orbit, which equations had not been even suspected by astronomers. The very remarkable agreement of this theory with all the ancient and modern observations has brought it to a very high degree of evidence.

The calculus of probabilities has led me similarly to the cause of the great irregularities of Jupiter and Saturn. Comparing modern observations with ancient, Halley found an acceleration in the movement of Jupiter and a retardation in that of Saturn. In order to conciliate the observations he reduced the movements to two secular equations of contrary signs and increasing as the squares of the times passed since 1700. Euler and Lagrange submitted to analysis the alterations which the mutual attraction of these two planets ought to produce in these movements. They found in doing this the secular equations; but their results were so different that one of the two at least ought to be erroneous. I determined then to take up again this important problem of *celestial mechanics*, and I recognized the invariability of the mean planetary movements, which nullified the secular equations introduced by Halley in the tables of Jupiter and Saturn. Thus there remain, in order to explain the great irregularity of these planets, only the attractions of the comets to which many astronomers had effective recourse, or the existence of an irregularity over a long period produced in the movements of the two planets by their reciprocal action and affected by contrary signs for each of them. A theorem which I found in regard to the inequalities of this kind rendered this inequality very probable. According to this theorem, if the movement of Jupiter is accelerated, that of Saturn is retarded, which has already conformed to what Halley had noticed; moreover, the acceleration of Jupiter resulting from the

same theorem is to the retardation of Saturn very nearly in the ratio of the secular equations proposed by Halley. Considering the mean movements of Jupiter and Saturn I was enabled easily to recognize that two times that of Jupiter differed only a very small quantity from five times that of Saturn. The period of an irregularity which would have for an argument this difference would be about nine centuries. Indeed its coefficient would be of the order of the cubes of the eccentricities of the orbits; but I knew that by virtue of successive integrations it acquired for divisor the square of the very small multiplier of the time in the argument of this inequality which is able to give it a great value; the existence of this inequality appeared to me then very probable. The following observation increased then its probability. Supposing its argument zero toward the epoch of the observations of Tycho Brahe, I saw that Halley ought to have found by the comparison of modern with ancient observations the alterations which he had indicated; while the comparison of the modern observations among themselves ought to offer contrary alterations similar to those which Lambert had concluded from this comparison. I did not then hesitate at all to undertake this long and tedious calculation necessary to assure myself of this inequality. It was entirely confirmed by the result of this calculation, which, moreover, made me recognize a great number of other inequalities of which the totality has inclined the tables of Jupiter and Saturn to the precision of the same observations.

It was again by means of the calculus of probabilities that I recognized the remarkable law of the mean movements of the three first satellites of Jupiter, according to which the mean longitude of the first, minus three times that of the second, plus two times that of the third is rigorously equal to the half-circumference. The approximation with which the mean movements of these [bodies] satisfy this law since their discovery, indicates its existence with an extreme probability. I sought then the cause of it in their mutual action. The searching examination of this action convinced me that it was sufficient if in the beginning the ratios of their mean movements had approached this law within certain limits, because their mutual action had established and maintained it rigorously. Thus, these three bodies will balance one another eternally in space according to the preceding law unless strange causes, such as comets, should change suddenly their movements about Jupiter.

OLBERS[1]

On the Determination of a Comet's Orbit

(Translated from "Abhandlung über die leichteste und bequemste Methode die Bahn eines Cometen aus einigen Beobachtungen zu berechnen," Weimar, 1797.)

1. To determine the orbit of a comet around the Sun from a few geocentric observations appeared even to the great Newton himself as not a little difficult. He called this problem *longe difficillimum*, the solution of which he had sought in various ways before he came upon the beautiful construction which he presents in his "Principles of Natural Philosophy." His construction is completely worthy of the genius of its creator; but it is seriously laborious and leads to the goal only after many approximations. Since Newton's time several of the greatest geometers have busied themselves with this problem, have proved or appreciated the impossibility of a direct, rigorously exact solution, and have given a great number of methods by which we can be led to knowledge of the elements of a comet's orbit. Some of these methods are shorter, others longer; some more, others less exact; indeed, various ones which their discoverers or other scholars had advanced as satisfactory and useful have been discarded again by other geometricians as completely useless. It seems therefore to be of interest to examine the comet problem again for its difficulties, and to bring all those methods into a general survey which serves to estimate their various values on the whole, in order to be able to select the shortest and most practicable way for determining the orbit of a comet.

2. Each geocentric observation of a comet gives the direction of a line of sight, somewhere along which the comet lay at the time of that observation. We may best consider two triangles in connection with each observation: one joining the centers of

[1] Heinrich Wilhelm Matthias Olbers (1758–1840), German astronomer, developed the first satisfactory method for determining cometary orbits. He was the discoverer of the asteroids Pallas and Vesta, and of several comets.

the sun, the comet, and the earth; and another joining the centers of the sun, of the earth, and of the projection of the comet on the plane of the ecliptic. From observation, one side only of each of these triangles is known—the distance of the earth from the sun; and one angle—the angle at the earth. To solve the triangles, in order to be able to fix the position of the comet, another side of each, or another angle, must be known. Both triangles will then be completely specified, since they are not independent. For the unknown quantity associated with each observation we may choose the angle at the comet, or at the sun; or the true distance of the comet from the earth, or from the sun; or the curtate distance.

3. Although comets almost never describe parabolic orbits about the sun, it is well known that we may safely treat as parabolic the small part of their elliptical path that lies close to the sun, and over which the comet is visible to us. I therefore assume the orbit of a comet to be a parabola with the center of the sun at the focus; all points in the comet's orbit then lie in a plane that passes through the center of the sun. If now we picture such a plane passing through the sun's center, each observation fixes the position of one line of sight, and, therefore, of one point in this plane. The parabola is fixed if only two points and the focus are given: if three observed points in the plane are to fall on a parabola, there is only one possible inclination corresponding to each assumed line of intersection with the ecliptic, and for any given inclination there is only one position of the line of nodes of that plane. Finally, four observations fix both the inclination and the line of nodes: and thus the orbit of a comet is completely specified by four observations, without any reference to time intervals, provided that it is parabolic.

4. Three observations would suffice if we considered the intervals, and assumed that the areas described about the sun were proportional to the times of description. But the areas are not simply proportional to the times; the times themselves are known functions of the *radii vectores* and the chords. Three observations are more than sufficient to specify the orbit of a comet: in other words we have in this case four equations and only three unknowns.

5. These four equations may easily be generalized. Let the three unknowns be the three distances of the comet from the earth. Three points not in the same straight line fix the position of a plane: consequently two distances and the center of the sun

fix both the position of this plane and the third distance. This gives the first equation. The requirement that the three positions of the comet must lie in a parabola with the sun at the focus gives the second equation. And, finally, the relation of the intervals to the *radii vectores* and the chords gives the two others. In general, when we have n observations we have n unknowns, and $3n - 5$ equations to determine them:—of these, $n - 2$ depend on the condition that all positions of the comet lie in a plane passing through the center of the sun; $n - 2$ are given by the parabolic path of the comet with the sun at the focus; and $n - 1$ by the known relation of the times to the chords and *radii vectores*.

6. It might perhaps seem to be a matter of no great difficulty, in view of this great redundancy of equations, to determine the orbit of a comet directly with geometrical accuracy from several geocentric observations. But when we examine the equations themselves it is evident that they are so complex that neither the resources of algebra nor the patience of the most assiduous computer would suffice for their solution. I shall now develop the four equations for the case of three observations, treating as unknown the curtate distances of the comet from the earth, the assumption that seems to me the most tractable . . .

19. The value of a method of computing the orbit of a comet must be estimated as jointly proportional to its brevity and to its accuracy. All methods give results that still require a correction, but the nearer the first result is to the truth, the easier will be this correction. If we apply these criteria to the method outlined in the third section, it will be found, I flatter myself, to have an advantage over all others. But first we must consider the equations of the first and second order which have been proposed for the solution of the cometary problem. If these equations were applicable in practice they would immediately eliminate the bother of seeking for another method or of selecting among those already available, for they would unquestionably furnish the simplest and most convenient method of determining the orbit of a comet.

BODE AND PIAZZI[1]

The Titius-Bode Law of Planetary Distances and the Discovery of Ceres

(Translated from Bode's, "Von dem neuen, zwischen Mars und Jupiter entdeckten achten Haupt planeten des Sonnensystems," Berlin, 1802.).

In the second edition of my Introduction to Knowledge of the Starry Heavens, which I published while yet in Hamburg, in the year 1772, I speak on page 462 concerning the probable existence of other planets in the solar system than had up to that time been known. Should the boundary of the solar system indeed be limited to where we see Saturn? (Since 1781 we know of Uranus at a distance double that of Saturn.) . . . and for what reason the great space which is found between Mars and Jupiter, where so far no major planet is seen? Is it not highly probable that a planet actually revolves in the orbit which the finger of the Almighty has drawn for it?

And in a note at this place: This conclusion appears to follow especially from the very remarkable relation which the six, long-known major planets observe in their distances from the Sun. If we indicate the distance of Saturn from the Sun by 100 units, Mercury is four such units from the Sun. Venus is $4 + 3 = 7$; the Earth, $4 + 6 = 10$; Mars, $4 + 12 = 16$. Now, however, there comes a gap in this regular progression. From Mars outward there follows a space of $4 + 24 = 28$ units in which, up to now, no planet has been seen. Can we believe that the Creator of the world has left this space empty? Certainly not! From here we come to the distance of Jupiter through $4 + 48 = 52$, and

[1] John Elert Bode (1747–1826), German astronomer, director of the Berlin Observatory, founded the *Astronomisches Jahrbuch*. The above account of the empirical rule of planetary intervals known as "Bode's law" shows how he gave currency to this rule, which was suggested (1766) by John Daniel Titius, professor at Wittenberg.

Giuseppe Piazzi (1746–1826), Italian astronomer, was the first director of the observatory at Palermo. He made one of the most important star catalogues of his time, and in the course of its preparation incidentally discovered Ceres, the first of the asteroids, of which more than a thousand are now known.

finally to Saturn through 4 + 96 = 100 units (and now to that of Uranus through 4 + 192 = 196 units) . . . This progression proceeds only in small numbers and, therefore, gives only approximate results. In all my subsequent astronomical writings I have, when occasion arose, spoken of this progression, presented it in sketches, and advanced many arguments for its correctness. The discovery of Uranus was the first happy verification of it.

This law in the increasing distance of the planets from the Sun does not lend itself freely to mathematical expression; it is merely empirical and would be inferred from analysis and conclusions, but it remains an iterated indication of the harmonious order which reigns everywhere in the great works of Nature.

I found the first idea of it in Bonnet's "Obervations Concerning Nature," translated by Titius, second edition, 1772, in a note by the translator on page 7. The original edition by Bonnet has nothing of it. It is noteworthy that as yet no mention has ever appeared of this progression in the astronomical work of foreigners. Only German astronomers have mentioned it after I drew attention to it in my astronomical writings.

The progression agrees very well with observations even in small numbers. If, however, we put with Professor Wurm (cf. Astronom. Jahrb. 1790, p. 168) the actual mean distance of Mercury from the Sun at 387 (the distance of the Earth = 1000) and take the distance between Mercury and Venus as 293, then the relative distances of the seven known planets are still more exactly represented. The distances from the Sun are in fact as follows:

		Mean distance
Mercury	387 units	387
Venus	387 + 293 = 680	723
The Earth	387 + 2 × 293 = 973	1,000
Mars	387 + 4 × 293 = 1,559	1,524
Probable planet between Mars and Jupiter	387 + 8 × 293 = 2,731	
Jupiter	387 + 16 × 293 = 5,075	5,203
Saturn	387 + 32 × 293 = 9,763	9,541
Uranus	387 + 64 × 293 = 19,139	19,082

On the 20th of March, 1801, I received from Dr. Joseph Piazzi, Royal Astronomer and Director of the Royal Observatory at Palermo, a communication dated January 24th in which he writes

as follows: "On the 1st of January I discovered a comet in Taurus in right ascension 51°47′, northern declination 16°8′. On the 11th it changed its heretofore (westward) retrograde motion into (eastward) direct motion; and on the 23d was in right ascension 51°46′, northern declination 17°8′. I shall continue to observe it and hope to be able to observe throughout the whole of February. It is very small, and equivalent to a star of the eighth magnitude, without any noticeable nebulosity. I beg of you to let me know whether it has already been observed by other astronomers; in this case I should save myself the trouble of computing its orbit."

In the beginning of March, I had already found a notice of the discovery in foreign journals; there was, however, as little said on the place and motion as on the appearance of this remarkable comet.

When, however, I received from the observer himself the foregoing more exact notice of the object, it struck me immediately, upon reading through his letter, as remarkable, and I was convinced that this small star without noticeable nebulosity, at one time in eastern elongation, then appearing to stand still, thereafter again moving forward toward the east, was not a comet at all; Piazzi had, indeed, here discovered a very extraordinary object. It was most probably the eighth major planet of the solar system, which already thirty years before I had announced between Mars and Jupiter, but which until now had remained undiscovered—a planet whose distance from the Sun indicated a known progression of probably 2.80, and which in four years and eight months must run its course around the Sun.

GAUSS[1]

Theory of the Motion of the Heavenly Bodies Moving about the Sun in Conic Sections

(From "Theoria Motus," 1809; translated by Charles Henry Davis, 1857.)

Introduction

To determine the orbit of a heavenly body, without any hypothetical assumption, from observations not embracing a great period of time, and not allowing a selection with a view to the application of special methods, was almost wholly neglected up to the beginning of the present century; or, at least, not treated by any one in a manner worthy of its importance . . .

Some ideas occurred to me in the month of September of the year 1801, engaged at the time on a very different subject, which seemed to point to the solution of the great problem of which I have spoken. Under such circumstances we not unfrequently, for fear of being too much led away by an attractive investigation, suffer the associations of ideas, which, more attentively considered, might have proved most fruitful in results, to be lost from neglect. And the same fate might have befallen these conceptions, had they not happily occurred at the most propitious moment for their preservation and encouragement that could have been selected. For just about this time the report of the new planet, discovered on the first day of January of that year with the telescope at Palermo, was the subject of universal conversation; and soon afterwards the observations made by that distinguished astronomer *Piazzi* from the above date to the eleventh of February were published. Nowhere in the annals of astronomy do we meet with so great an opportunity, and a greater one could hardly be imagined, for showing most strikingly, the value of this problem, than in this crisis and urgent necessity, when all hope of discovering in the heavens this planetary atom, among innumerable small stars after

[1] Carl Friedrich Gauss (1777–1855), German mathematician and director of the observatory at Göttingen, invented the method of least squares and used it in his epoch-making work on orbit theory.

the lapse of nearly a year, rested solely upon a sufficiently approximate knowledge of its orbit to be based upon these very few observations. Could I ever have found a more seasonable opportunity to test the practical value of my conceptions, than now in employing them for the determination of the orbit of the planet Ceres, which during these forty-one days had described a geocentric arc of only three degrees, and after the lapse of a year must be looked for in a region of the heavens very remote from that in which it was last seen? The first application of the method was made in the month of October, 1801, and the first clear night, when the planet was sought for as directed by the numbers deduced from it, restored the fugitive to observation. Three other new planets, subsequently discovered, furnished new opportunities for examining and verifying the efficiency and generality of the method.

Determination of an Orbit from Three Complete Observations

Seven elements are required for the complete determination of the motion of a heavenly body in its orbit, the number of which, however, may be diminished by one, if the mass of the heavenly body is either known or neglected; neglecting the mass can scarcely be avoided in the determination of an orbit wholly unknown, where all the quantities of the order of the perturbations must be omitted, until the masses on which they depend become otherwise known. Wherefore, in the present inquiry, the mass of the body being neglected, we reduce the number of the elements to six, and, therefore, it is evident, that as many quantities depending on the elements, but independent of each other, are required for the determination of the unknown orbit. These quantities are necessarily the places of the heavenly body observed from the earth; since each one of which furnishes two data, that is, the longitude and latitude, or the right ascension and declination, it will certainly be the most simple to adopt *three geocentric places* which will, in general, be sufficient for determining the six unknown elements. This problem is to be regarded as the most important in this work, and, for this reason, will be treated with the greatest care in this section.

But in the special case, in which the plane of the orbit coincides with the ecliptic, and thus both the heliocentric and geocentric latitudes, from their nature, vanish, the three vanishing geocentric

latitudes cannot any longer be considered as three data independent of each other: then, therefore, this problem would remain indeterminate, and the three geocentric places might be satisfied by an infinite number of orbits. Accordingly, in such a case, four geocentric longitudes must, necessarily, be given, in order that the four remaining unknown elements (the inclination of the orbit and the longitude of the node being omitted) may be determined. But although, from an indiscernible principle, it is not to be expected that such a case would ever actually present itself in nature, nevertheless, it is easily imagined that the problem, which, in an orbit exactly coinciding with the plane of the ecliptic, is absolutely indeterminate, must, on account of the limited accuracy of the observations, remain nearly indeterminate in orbits very little inclined to the ecliptic, where the very slightest errors of the observations are sufficient altogether to confound the determination of the unknown quantities. Wherefore, in order to examine this case, it will be necessary to select six data; for which purpose we will show in section second, how to determine an unknown orbit from four observations, of which two are complete, but the other two incomplete, the latitudes or declinations being deficient.

Finally, as all our observations, on account of the imperfection of the instruments and of the senses, are only approximations to the truth, an orbit based only on the six absolutely necessary data may be still liable to considerable errors. In order to diminish these as much as possible, and thus to reach the greatest precision attainable, no other method will be given except to accumulate the greatest number of the most perfect observations, and to adjust the elements, not so as to satisfy this or that set of observations with absolute exactness, but so as to agree with all in the best possible manner. For which purpose, we will show in the third section how, according to the principles of the calculus of probabilities, such an agreement may be obtained, as will be, if in no one place perfect, yet in all the places the strictest possible.

The determination of orbits in this manner, therefore, so far as the heavenly bodies move in them according to the laws of *Kepler*, will be carried to the highest degree of perfection that is desired. Then it will be proper to undertake the final correction, in which the perturbations that the other planets cause in the motion, will be taken account of: we will indicate briefly in the fourth section, how these may be taken account of, so far at least, as it shall appear consistent with our plan.

Before the determination of any orbit from geocentric observations, if the greatest accuracy is desired, certain reductions must be applied to the latter on account of nutation, precession, parallax, and aberration: these small quantities may be neglected in the rougher calculation . . .

It would not be difficult, from the connection between the data and unknown quantities of our problem, to reduce its statement to six equations, or even to less, since one or another of the unknown quantities might, conveniently enough, be eliminated: but since this connection is most complicated, these equations would become very intractable; such a separation of the unknown quantities as finally to produce an equation containing only one, can, generally speaking, be regarded as impossible, and, therefore, still less will it be possible to obtain a complete solution of the problem by direct processes alone.

But our problem may at least be reduced, and that too in various ways, to the solution of *two* equations $X = 0$, $Y = 0$, in which only two unknown quantities x, y, remain. It is by no means necessary that x, y, should be two of the elements: they may be quantities connected with the elements in any manner whatever, if, only, the elements can be conveniently deduced from them when found. Moreover, it is evidently not requisite that X, Y, be expressed in explicit functions of x, y: it is sufficient if they are connected with them by a system of equations in such manner that we can proceed from given values of x, y, to the corresponding values of X, Y . . .

The ten methods explained from Article 124 forwards, rest upon the assumption that approximate values of the distances of the heavenly body from the earth, or of the position of the plane of the orbit, are already known. When the problem is, to correct, by means of observations more remote from each other, the dimensions of an orbit, the approximate values of which are already, by some means, known, as, for instance, by a previous calculation based on other observations, this assumption will evidently be liable to no difficulty. But it does not as yet appear from this, how the first calculation is to be entered upon when all the dimensions of the orbit are still wholly unknown: this case of our problem is by far the most important and the most difficult, as may be imagined from the analogous problem in the theory of comets, which, as is well known, has perplexed geometers for a long time,

and has given rise to many fruitless attempts. In order that our problem may be considered as correctly solved, that is, if the solution be given in accordance with what has been explained in the 119th and subsequent articles, it is evidently requisite to satisfy the following conditions:—*First*, the quantities x, y, are to be chosen in such a manner, that we can find approximate values of them from the very nature of the problem, at all events, as long as the heliocentric motion of the heavenly body between the observations is not too great. *Secondly*, it is necessary that, for small changes in the quantities x, y, there be not too great corresponding changes in the quantities to be derived from them, lest the errors accidentally introduced in the assumed values of the former, prevent the latter from being considered as approximate. *Thirdly* and lastly, we require that the processes by which we pass from the quantities x, y, to X, Y, successively, be not too complicated.

These conditions will furnish the criterion by which to judge of the excellence of any method: this will show itself more plainly by frequent applications. The method which we are now prepared to explain, and which, in a measure, is to be regarded as the most important part of this work, satisfies these conditions so that it seems to leave nothing further to be desired . . .

The method which we have fully explained is principally suited to the first determination of a wholly unknown orbit: still it is employed with equally great success, where the object is the correction of an orbit already approximately known by means of three observations however distant from each other . . .

Determination of an Orbit from Any Number of Observations

From these general discussions we return to our special subject for the sake of which they were undertaken. Before the most accurate determination of the orbit from more observations than are absolutely requisite can be commenced, there should be an approximate determination which will nearly satisfy all the given observations. The corrections to be applied to these approximate elements, in order to obtain the most exact agreement, will be regarded as the objects of the problem. And when it can be assumed that these are so small that their squares and products may be neglected, the corresponding changes, produced in the computed geocentric places of a heavenly body, can be obtained

by means of the differential formulas given in the Second Section of the First Book. The computed places, therefore, which we obtain from the corrected elements, will be expressed by linear functions of the corrections of the elements, and their comparison with the observed places according to the principles before explained, will lead to the determination of the most probable values. These processes are so simple that they require no further illustration, and it appears at once that any number of observations, however remote from each other, can be employed. The same method may also be used in the correction of the *parabolic* orbits of comets, should we have a long series of observations and the best agreement be required.

The Method of Least Squares

The investigation of an orbit having, strictly speaking, the *maximum* probability, will depend upon a knowledge of the law according to which the probability of errors decreases as the errors increase in magnitude: but that depends upon so many vague and doubtful considerations—physiological included—which cannot be subjected to calculation, that it is scarcely, and indeed less than scarcely, possible to assign properly a law of this kind in any case of practical astronomy. Nevertheless, an investigation of the connection between this law and the most probable orbit, which we will undertake in its utmost generality, is not to be regarded as by any means a barren speculation.

To this end let us leave our special problem, and enter upon a very general discussion and one of the most fruitful in every application of the calculus to natural philosophy. Let V, V', V'', etc. be functions of the unknown quantities p, q, r, s, etc., μ the number of those functions, ν the number of the unknown quantities; and let us suppose that the values of the functions found by direct observation are $V = M$, $V' = M'$, $V'' = M''$, etc. Generally speaking, the determination of the unknown quantities will constitute a problem, indeterminate, determinate, or more than determinate, according as $\mu < \nu$, $\mu = \nu$, or $\mu > \nu$. We shall confine ourselves here to the last case, in which, evidently, an exact representation of all the observations would only be possible when they were all absolutely free from error. And since this cannot, in the nature of things, happen, every system of values of the unknown

quantities p, q, r, s, etc., must be regarded as possible, which gives the values of the functions $V - M$, $V' - M'$, $V'' - M''$, etc., within the limits of the possible errors of observation; this, however, is not to be understood to imply that each one of these systems would possess an equal degree of probability.

Let us suppose, in the first place, the state of things in all the observations to have been such, that there is no reason why we should suspect one to be less exact than another, or that we are bound to regard errors of the same magnitude as equally probable in all. Accordingly, the probability to be assigned to each error Δ will be expressed by a function of Δ which we shall denote by $\varphi\Delta$. Now although we cannot precisely assign the form of this function, we can at least affirm that its value should be a maximum for $\Delta = 0$, equal, generally, for equal opposite values of Δ, and should vanish, if, for Δ is taken the greatest error, or a value greater than the greatest error: $\varphi\Delta$, therefore, would appropriately be referred to the class of discontinuous functions, and if we undertake to substitute any analytical function in the place of it for practical purposes, this must be of such a form that it may converge to zero on both sides, asymptotically, as it were, from $\Delta = 0$, so that beyond this limit it can be regarded as actually vanishing. Moreover, the probability that an error lies between the limits Δ and $\Delta + d\Delta$ differing from each other by the infinitely small difference $d\Delta$, will be expressed by $\varphi\Delta d\Delta$; hence the probability generally, that the error lies between D and D', will be given by the integral $\int\varphi\Delta.d\Delta$ extended from $\Delta = D$ to $\Delta = D'$. This integral taken from the greatest negative value of Δ to the greatest positive value, or more generally from $\Delta = -\infty$ to $\Delta = +\infty$ must necessarily be equal to unity. Supposing, therefore, any determinate system of the values of the quantities p, q, r, s, etc., the probability that observation would give for V the value M, will be expressed by $\varphi(M - V)$, substituting in V for p, q, r, s, etc., their values; in the same manner $\varphi(M' - V')$, $\varphi(M'' - V'')$, etc. will express the probabilities that observation would give the values M', M'', etc., of the functions V', V'', etc. Wherefore, since we are authorized to regard all the observations as events independent of each other, the product

$$\varphi(M - V)\varphi(M' - V')\varphi(M'' - V'') \text{ etc., } = \Omega$$

will express the expectation or probability that all those values will result together from observation.

Now in the same manner as, when any determinate values whatever of the unknown quantities being taken, a determinate probability corresponds, previous to observation, to any system of values of the functions V, V', V'', etc.; so, inversely, after determinate values of the functions have resulted from observation, a determinate probability will belong to every system of values of the unknown quantities, from which the values of the functions could possibly have resulted: for, evidently, those systems will be regarded as the more probable in which the greater expectation had existed of the event which actually occurred. The estimation of this probability rests upon the following theorem:—

If, any hypothesis H *being made, the probability of any determinate event* E *is* h, *and if, another hypothesis* H′ *being made excluding the former and equally probable in itself, the probability of the same event is* h′: *then I say, when the event* E *has actually occurred, that the probability that* H *was the true hypothesis, is to the probability that* H′ *was the true hypothesis, as* h to h′.

For demonstrating which let us suppose that, by a classification of all the circumstances on which it depends whether, with H or H' or some other hypothesis, the event E or some other event, should occur, a system of the different cases is formed, each one of which cases is to be considered as equally probable in itself (that is, as long as it is uncertain whether the event E, or some other, will occur), and that these cases be so distributed,

that among them may be found	in which should be assumed the hypothesis	in such a mode as would give occasion to the event
m	H	E
n	H	different from E
m'	H'	E
n'	H'	different from E
m''	different from H and H'	E
n''	different from H and H'	different from E

Then we shall have

$$h = \frac{m}{m+n}, \; h' = \frac{m'}{m'+n'};$$

moreover, before the event was known the probability of the hypothesis H was

$$\frac{m+n}{m+n+m'+n'+m''+n''},$$

but after the event is known, when the cases n, n', n'' disappear from the number of the possible cases, the probability of the same hypothesis will be

$$\frac{m}{m + m' + m''};$$

in the same way the probability of the hypothesis H' before and after the event, respectively, will be expressed by

$$\frac{m' + n'}{m + n + m' + n' + m'' + n''} \text{ and } \frac{m'}{m + m' + m''};$$

since, therefore, the same probability is assumed for the hypotheses H and H' before the event is known, we shall have

$$m + n = m' + n',$$

whence the truth of the theorem is readily inferred.

Now, so far as we suppose that no other data exist for the determination of the unknown quantities besides the observations $V = M$, $V' = M'$, $V'' = M''$, etc., and, therefore, that all systems of values of these unknown quantities were equally probable previous to the observations, the probability, evidently, of any determinate system subsequent to the observations will be proportional to Ω. This is to be understood to mean that the probability that the values of the unknown quantities lie between the infinitely near limits p and $p + \mathrm{d}p$, q and $q + \mathrm{d}q$, r and $r + \mathrm{d}r$, s and $s + \mathrm{d}s$, etc. respectively, is expressed by

$$\lambda\Omega \mathrm{d}p\mathrm{d}q\mathrm{d}r\mathrm{d}s \ . \ . \ . \text{, etc.},$$

where the quantity λ will be a constant quantity independent of p, q, r, s, etc.: and, indeed, $\frac{1}{\lambda}$ will, evidently, be the value of the integral of the order ν,

$$\int \nu\Omega \mathrm{d}p\mathrm{d}q\mathrm{d}r\mathrm{d}s \ . \ . \ . \text{, etc.},$$

for each of the variables p, q, r, s, etc., extended from the value $-\infty$ to the value $+\infty$.

Now it readily follows from this, that the most probable system of values of the quantities p, q, r, s, etc. is that in which Ω acquires the maximum value, and, therefore, is to be derived from the ν equations

$$\frac{\mathrm{d}\Omega}{\mathrm{d}p} = 0, \frac{\mathrm{d}\Omega}{\mathrm{d}q} = 0, \frac{\mathrm{d}\Omega}{\mathrm{d}r} = 0, \frac{\mathrm{d}\Omega}{\mathrm{d}s} = 0, \text{ etc.}$$

These equations, by putting

$$V - M = v,\ V' - M' = v',\ V'' - M'' = v'',\ \text{etc., and}\ \frac{\mathrm{d}\varphi\Delta}{\varphi\Delta\mathrm{d}\Delta} = \varphi'\Delta,$$

assume the following form:—

$$\begin{aligned}
&\frac{\mathrm{d}v}{\mathrm{d}p}\varphi'v + \frac{\mathrm{d}v'}{\mathrm{d}p}\varphi'v' + \frac{\mathrm{d}v''}{\mathrm{d}p}\varphi'v'' + \text{etc.} = 0,\\
&\frac{\mathrm{d}v}{\mathrm{d}q}\varphi'v + \frac{\mathrm{d}v'}{\mathrm{d}q}\varphi'v' + \frac{\mathrm{d}v''}{\mathrm{d}q}\varphi'v'' + \text{etc.} = 0,\\
&\frac{\mathrm{d}v}{\mathrm{d}r}\varphi'v + \frac{\mathrm{d}v'}{\mathrm{d}r}\varphi'v' + \frac{\mathrm{d}v''}{\mathrm{d}r}\varphi'v'' + \text{etc.} = 0,\\
&\frac{\mathrm{d}v}{\mathrm{d}s}\varphi'v + \frac{\mathrm{d}v'}{\mathrm{d}s}\varphi'v' + \frac{\mathrm{d}v''}{\mathrm{d}s}\varphi'v'' + \text{etc.} = 0.
\end{aligned}$$

Hence, accordingly, a completely determinate solution of the problem can be obtained by elimination, as soon as the nature of the function φ' is known. Since this cannot be defined *a priori*, we will, approaching the subject from another point of view, inquire upon what function, tacitly, as it were, assumed as a base, the common principle, the excellence of which is generally acknowledged, depends. It has been customary certainly to regard as an axiom the hypothesis that if any quantity has been determined by several direct observations, made under the same circumstances and with equal care, the arithmetical mean of the observed values affords the most probable value, if not rigorously, yet very nearly at least, so that it is always most safe to adhere to it. By putting, therefore,

$$V = V' = V'' \text{ etc.} = p,$$

we ought to have in general,

$$\varphi'(M - p) + \varphi'(M' - p) + \varphi'(M'' - p) + \text{etc.} = 0,$$

if instead of p is substituted the value

$$\frac{1}{\mu}(M + M' + M'' + \text{etc.}),$$

whatever positive integer μ expresses. By supposing, therefore,

$$M' = M'' = \text{etc.} = M - \mu N,$$

we shall have in general, that is, for any positive integral value of μ,

$$\varphi'(\mu - 1)N = (1 - \mu)\varphi'(-N),$$

whence it is readily inferred that $\frac{\phi'\Delta}{\Delta}$ must be a constant quantity, which we will denote by k. Hence we have

$$\log \varphi\Delta = \tfrac{1}{2}\, k\Delta\Delta + \text{Constant},$$
$$\varphi\Delta = \varkappa e^{\frac{1}{2}k\Delta\Delta}.$$

denoting the base of the hyperbolic logarithms by e and assuming

$$\text{Constant} = \log \varkappa.$$

Moreover, it is readily perceived that k must be negative, in order that Ω may really become a maximum, for which reason we shall put

$$\tfrac{1}{2}k = -bb;$$

and since, by the elegant theorem first discovered by LAPLACE, the integral

$$\int e^{-hh\Delta\Delta} d\Delta$$

from $\Delta = -\infty$ to $\Delta = +\infty$ is $\frac{\sqrt{\pi}}{h}$, (denoting by π the semicircumference of the circle the radius of which is unity), our function becomes

$$\varphi\Delta = \frac{h}{\sqrt{\pi}} e^{-hh\Delta\Delta}.$$

The function just found cannot, it is true, express rigorously the probabilities of the errors: for since the possible errors are in all cases confined within certain limits, the probability of errors exceeding those limits ought always to be zero, while our formula always gives some value. However, this defect, which every analytical function must, from its nature, labor under, is of no importance in practice, because the value of our function decreases so rapidly, when $h\Delta$ has acquired a considerable magnitude, that it can safely be considered as vanishing. Besides, the nature of the subject never admits of assigning with absolute rigor the limits of error.

Finally, the constant h can be considered as the measure of precision of the observations. For if the probability of the error Δ is supposed to be expressed in any one system of observations by

$$\frac{h}{\sqrt{\pi}} e^{-hh\Delta\Delta},$$

and in another system of observations more or less exact by

$$\frac{h'}{\sqrt{\pi}} e^{-h'h'\Delta\Delta},$$

the expectation, that the error of any observation in the former system is contained between the limits $-\delta$ and $+\delta$ will be expressed by the integral

$$\int \frac{h}{\sqrt{\pi}} e^{-hh\Delta\Delta} d\Delta$$

taken from $\Delta = -\delta$ to $\Delta = +\delta$; and in the same manner the expectation, that the error of any observation in the latter system does not exceed the limits $-\delta'$ and $+\delta'$ will be expressed by the integral

$$\int \frac{h'}{\sqrt{\pi}} e^{-h'h'\Delta\Delta} d\Delta$$

extended from $\Delta = -\delta'$ to $\Delta = +\delta'$: but both integrals manifestly become equal when we have $h\delta = h'\delta'$. Now, therefore, if for example $h' = 2h$, a double error can be committed in the former system with the same facility as a single error in the latter, in which case, according to the common way of speaking, a double degree of precision is attributed to the latter obervations.

We will now develop the conclusions which follow from this law. It is evident, in order that the product

$$\Omega = h^{\mu}\pi^{-\frac{1}{2}\mu} e^{-hh(vv+v'v'+v''v''+\cdots)}$$

may become a maximum, that the sum

$$vv + v'v' + v''v'' + \text{etc.},$$

must become a minimum. *Therefore, that will be the most probable system of values of the unknown quantities* p, q, r, s, *etc., in which the sum of the squares of the differences between the observed and computed values of the functions* V, V′, V″, *etc. is a minimum,* if the same degree of accuracy is to be presumed in all the observations. This principle, which promises to be of most frequent use in all applications of the mathematics to natural philosophy, must, everywhere, be considered an axiom with the same propriety as the arithmetical mean of several observed values of the same quantity is adopted as the most probable value.

This principle can be extended without difficulty to observations of *unequal* accuracy. If, for example, the measures of precision of the observations by means of which $V = M$, $V' = M'$, $V'' = M''$, etc. have been found, are expressed, respectively, by h, h', h'', etc., that is, if it is assumed that errors reciprocally proportional to these quantities might have been made with equal facility in

those observations, this, evidently, will be the same as if, by means of observations of equal precision (the measure of which is equal to unity), the values of the functions bV, $b'V'$, $b''V''$, etc., had been directly found to be bM, $b'M'$, $b''M''$, etc.: wherefore, the most probable system of values of the quantities p, q, r, s, etc., will be that in which the sum of $bbvv + b'b'v'v' + b''b''v''v'' +$ etc., that is, *in which the sum of the squares of the differences between the actually observed and computed values multiplied by numbers that measure the degree of precision, is a minimum.* In this way it is not even necessary that the functions V, V', V'', etc. relate to homogeneous quantities, but they may represent heterogeneous quantities also, (for example, seconds of arc and time), provided only that the ratio of the errors, which might have been committed with equal facility in each, can be estimated.

FRAUNHOFER[1]

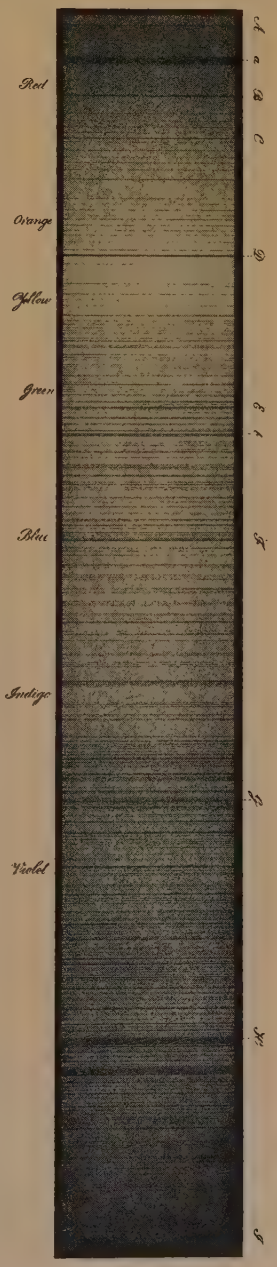
Discovery and Description of Lines in the Solar Spectrum

(From "Prismatic and Diffraction Spectra," translated by J. S. Ames, 1898, by arrangement with the American Book Company, publishers.)

In the window-shutter of a darkened room I made a narrow opening—about 15 seconds broad and 36 minutes high—and through this I allowed sunlight to fall on a prism of flint-glass which stood upon the theodolite described before. The theodolite was 24 feet from the window, and the angle of the prism was about 60°. The prism was so placed in front of the objective of the theodolite-telescope that the angle of incidence of the light was equal to the angle at which the beam emerged. I wished to see if in the color-image from sunlight there was a bright band similar to that observed in the color-image of lamplight. But instead of this I saw with the telescope an almost countless number of strong and weak vertical lines, which are, however, darker than the rest of the color-image; some appeared to be almost perfectly black. If the prism was turned so as to increase the angle of incidence, these lines vanished; they disappear also if the angle of incidence is made smaller. For increased angle of incidence, however, these lines becomes visible again if the telescope is made shorter; while, for a smaller angle of incidence, the eye-piece must be drawn out considerably in order to make the lines reappear. If the eye-piece was so placed that the lines in the red portion of the color-image could be plainly seen, then, in order to see the lines in the violet portion, it must be pushed in slightly. If the opening through which the light entered was made broader, the fine lines ceased to be clearly seen, and vanished entirely if the opening was made 40 seconds wide. If the opening was made 1 minute wide, even the broad lines could not be seen plainly. The distances apart of the lines, and all their relations to each other, remained unchanged,

[1] Joseph Fraunhofer (1787–1826), German optician, discovered the Fraunhofer lines in the solar spectrum while engaged in perfecting the construction of achromatic lenses. The above excerpts were published in 1817 in *Denkschriften der königlichen Akademie der Wissenschaften zu München;* and in *Edinburgh Journal of Science,* 1827, 1828.

both when the width of the opening in the window-shutter was altered and when the distance of the theodolite from the opening was changed. The prism could be of any kind of refractive material, and its angle might be large or small; yet the lines remained always visible, and only in proportion to the size of the color-image did they become stronger or weaker, and therefore were observed more easily or with more difficulty.



The relations of these lines and streaks among themselves appeared to be the same with every refracting substance; so that, for instance, one particular band is found in every case only in the blue; another is found only in the red; and one can, therefore, at once recognize which line he is observing. These lines can be recognized also in the spectra formed by both the ordinary and the extraordinary rays of Iceland spar. The strongest lines do not in any way mark the limits of the various colors; there is almost always the same color on both sides of a line, and the passage from one color into another cannot be noted.

With reference to these lines the color-image is as shown in the accompanying figure. It is, however, impossible to show on this scale all the lines and their intensities. (The red end of the color-image is in the neighborhood of *A*; the violet end is near *I*.) It is, however, impossible to set a definite limit at either end, although it is easier at the red than at the violet. Direct sunlight, or sunlight reflected by a mirror, seems to have its limits on the one hand, somewhere between *G* and *H*; on the other, at *B*; yet with sunlight of great intensity the color-image becomes half again as long. In

order, however, to see this great spreading-out of the spectrum, the light from the space between *C* and *G* must be prevented from entering the eye, because the impression which the light from the extremities of the color-image makes upon the eye is very weak, and is destroyed by the rest of the light. At *A* there is easily recognized a sharply defined line; yet this is not the limit of the red color, for it proceeds much beyond. At *A* there are heaped together many lines which form a band; *B* is sharply defined and is of noticeable thickness. In the space between *B* and *C* there can be counted 9 very fine, sharply defined lines. The line *C* is of considerable strength, and, like *B*, is very black. In the space between *C* and *D* there can be counted 30 very fine lines; but these (with two exceptions), like those between *B* and *C*, can be plainly seen only with strong magnification or with prisms which have great dispersion; they are, moreover, very sharply defined. *D* consists of two strong lines which are separated by a bright line. Between *D* and *E* there can be counted some 84 lines of varying intensities. *E* itself consists of several lines, of which the one in the middle is somewhat stronger than the rest. Between *E* and *b* are about 24 lines. At *b* there are 3 very strong lines, two of which are separated by only a narrow bright line; they are among the strongest lines in the spectrum. In the space between *b* and *F* there can be counted about 52 lines; *F* is fairly strong. Between *F* and *G* there are about 185 lines of different strengths. At *G* there are massed together many lines, among which several are distinguished by their intensity. In the space between *G* and *H* there are about 190 lines, whose intensities differ greatly. The two bands at *H* are most remarkable; they are almost exactly equal, and each consists of many lines; in the middle of each there is a strong line which is very black. From *H* to *I* the lines are equally numerous. . . .

I have convinced myself by many experiments and by varying the methods that these lines and bands are due to the nature of sunlight, and do not arise from diffraction, illusion, etc. If light from a lamp is allowed to pass through the same narrow opening in the window-shutter, none of these lines are observed, only the bright line *R*, which, however, comes exactly in the same place as the line *D*, so that the indices of refraction of the rays *D* and *R* are the same. The reason why the lines fade away, or even entirely vanish, when the opening at the window is made too wide is not difficult to give. The stronger lines have a width of from five to

ten seconds; so, if the opening of the window is not so narrow that the light which passes through can be regarded as belonging to one ray, or if the angular width of the opening is much more than that of the line, the image of one and the same line is repeated several times side by side, and consequently becomes indistinct, or vanishes entirely if the opening is made too wide.

Spectra of Venus and Sirius

I applied this form of apparatus at night-time to observe Venus directly, *without making the light pass through a small opening;* and I discovered in the spectrum of this light the same lines as those which appear in sunlight. Since, however, the light from Venus is feeble in comparison with sunlight reflected from a mirror, the intensity of the violet and the extreme red rays are very weak; and on this account even the stronger lines in both these colors are recognized only with difficulty, but in the other colors they are very easily distinguished. I have seen the lines *D*, *E*, *b*, *F* perfectly defined, and have even recognized that *b* consists of two lines, one weak and one strong; but the fact that the stronger one itself consists of two I could not verify owing to lack of light. For the same reason the other finer lines could not be distinguished satisfactorily. I have convinced myself by an approximate measurement of the arcs *DE* and *EF* that the light from Venus is in this respect of the same nature as sunlight.

With this same apparatus I made observations also on the light of some fixed stars of the first magnitude. Since, however, the light of these stars is much weaker than that of Venus, it is natural that the brightness of the spectrum should be much less. In spite of this I have seen with certainty in the spectrum of Sirius three broad bands which appear to have no connection with those of sunlight; one of these bands is in the green, two are in the blue. In the spectra of other fixed stars of the first magnitude one can recognize bands; yet these stars, with respect to these bands, seem to differ among themselves. Since the objective of the telescope has an aperture of only 13 lines [1 centimetre equals 4.43296 lines], it is clear that these observations can be repeated with much greater accuracy. I intend to repeat them with suitable alterations, and with a larger objective, in order to induce, perhaps, some skilled investigator to continue the experiments. Such a con-

tinuation is all the more to be desired, because the experiments would serve at the same time for the accurate comparison of the refraction of the light of the fixed stars with that of sunlight.

Spectra of the Moon and Starlight

As is well known, the prismatic *color-spectrum* of the light coming from a *flame* (*lamplight*) does not show the dark fixed lines which are present in the spectrum of sunlight; instead of them there is in the orange a bright line which is prominent above the rest of the spectrum, is double, and is at the same place where in sunlight the double line *D* is found. The spectrum obtained from the light of a flame which is blown with a *blast-tube* contains several prominent bright lines. Of still greater interest for optical experiments is the fact that, by skilful blowing of the flame, the light of the *front half* of the flame can be dispersed no further by the prism, and, consequently, is simple homogeneous light. This light has, so far as I have investigated it, the same refrangibility as the *D* ray of sunlight. Simple homogeneous light which proceeds in all directions is, for known reasons, very difficult to produce, and can never be obtained with prisms directly; therefore, this flame is of great use in many experiments . . .

The light of the *moon* gave me a spectrum which showed in the brightest colors the same fixed lines as did sunlight, and in exactly the same places.

To observe the *spectra of the light of the fixed stars*, and at the same time to *determine the refrangibility of this light*, I prepared a short time ago a suitable apparatus specially adapted to this end, the telescope belonging to it having an objective of 4 inches' aperture . . .

Up to the present we have found no *fixed star* whose light, so far as its *refrangibility* is concerned, is sensibly different from that of the *planets*. When the fixed lines of the spectra are seen plainly, one can be certain with this instrument to 10 seconds; and when the fixed lines cannot be seen, one can still be certain for the *orange light* to ½ minute. Since the total refraction through the prism is 26°, a difference amounting to $\frac{1}{9360}$ of the whole refraction could still be noticed with this instrument, a difference which even with the horizontal refraction in the atmosphere did not amount to ¼ second. Up to this time, as is well known, some astronomers have

doubted whether the refraction tables for different stars should not be somewhat different; therefore, this doubt seems to be removed by the experiment noted. The continuation of this investigation will lead us, I hope, to more complete knowledge.

In order to see the *fixed lines* of the different stars (with this large instrument) the air must be most favorable—a condition which happens rarely to a sufficient extent. The spectra of the light from *Mars* and *Venus* contain the same fixed lines as does sunlight, and in exactly the same places, at least so far as the lines *D*, *E*, *b*, and *F* are concerned, whose relative positions can be exactly determined. In the spectrum of the light from *Sirius* I could not distinguish fixed lines in the orange and yellow; in the green, however, there is seen a very strong streak; and in the blue there are two other unusually strong streaks, which seem to be unlike any of the lines of planetary light. We have determined their positions with the micrometer. *Castor* gives a spectrum which is like that of Sirius; the streak in the green, in spite of the weak light, was intense enough for me to be able to measure it; and I found it in exactly the same place as it was with Sirius. I could also distinguish the streaks in the blue; but the light was too feeble to allow of measurement. In the spectrum of *Pollux*, I recognized many fixed lines which resembled those of Venus; but all were weak. I saw the *D* line quite plainly, in exactly the same position as with planetary light. *Capella* gives a spectrum in which, at the places *D* and *b*, the same fixed lines are seen as in sunlight. The spectrum of *Betelgeux* (α Orionis) contains countless fixed lines which, with a good atmosphere, are sharply defined; and, although at first sight it seems to have no resemblance to the spectrum of Venus, yet similar lines are found in the spectrum of this fixed star in exactly the places where with sunlight *D* and *b* come. Some lines can be distinguished in the spectrum of *Procyon;* but they are seen with difficulty, and so indistinctly that their positions cannot be determined with certainty. I think I saw a line at the position *D* in the orange.

AIRY[1]

The Figure of the Earth

(From "Encyclopædia Metropolitana," Vol. 5, 1845.)

Since Astronomy first assumed the form of a Science, the inquiry into the Figure and dimensions of the Earth has always excited the interest of Philosophers. It can hardly be doubted that in the mind of a reflecting man there would always be a desire to know the nature of the Planet upon which he existed; but without Science of an exalted order, it would be impossible for him to gratify his curiosity. For the Astronomer, however, it was absolutely necessary to know something of the form of the Earth; and Astronomy alone gave the means of determining it. While the only observations of the heavenly bodies consisted in watching the rising and setting of the Sun and Stars, the form of the Earth was a matter of indifference; but when an attempt was made to reduce to a system the motions of the luminaries and Planets, it was necessary as a preliminary step to assume, and even to establish by something approaching to demonstration, a hypothesis on the Figure of the Earth.

At present the determination of the Earth's Figure possesses an interest of which formerly it was altogether destitute. The Astronomers of antiquity knew that the Earth is nearly spherical; they had also some not very correct ideas of its magnitude, and this was sufficient for their purposes. But the sphericity of the Earth, when thus established, was an isolated fact. It was proved by the impossibility of explaining certain phenomena on any other hypothesis; but it was not connected with any general theory which made this Figure a necessary consequence of the properties of matter. But by the discoveries of Newton the Figure of the Earth was shown to depend on the same theory which explains with such wonderful accuracy the motions of the Planets and their

[1] George Biddell Airy (1801–1892), English Astronomer Royal from 1835 to 1881. He is known for his work on achromatic eye-pieces, the discovery of astigmatism of the human eye, and for important observational and theoretical work in many fields of astronomy.

satellites. The investigations of the most profound Mathematicians have since been directed to its determination, from the principles of Gravitation; and the labours of the most able experimenters have been employed in ascertaining it from actual observation; and the comparison of the results of theory and of observation shows that their agreement, though not perfectly exact, is sufficiently so to enable us to assert with confidence, that the Principle of Gravitation is well founded. Indeed, for one part of that Principle (*viz.* that the attraction of a Planet is not a force directed to its centre, but is the resultant of all the forces directed to every one of its particles), it may be considered as affording the most satisfactory proof that we can expect ever to have . . .

History.—In the Astronomical Works of Ptolemy (who flourished about A.D. 137) we find no notice of the Earth's dimensions. He assigns as reasons for believing the Earth to be spherical, that eclipses of the Moon, as seen at different places on the Earth, take place at different times with reference to the noon of the places of observation; and that the differences of apparent times are proportional to the distances East or West. This, we believe, is the earliest notice of the Principle of deducing terrestrial longitude from the difference of the apparent times at which an eclipse of the Moon is observed. He remarks also that on going Northward, the number of circumpolar stars is increased, appearances which could not exist if the Earth were plane, or cylindrical. But in his Geography he tacitly assumes the Earth to be spherical, and uses constantly as the length of one degree 500 stadia. This estimation seems to have been made by Marinus, the Tyrian, from observations of the latitude of very distant places, and from the rough measures of the distance made by sailors and merchants . . .

In 1669 the trigonometrical measure of Picard was commenced; and it was in every respect superior to all that had proceeded it. It is true that it was not free from errors: an error of six toises was committed in the measure of the base; and his differences of latitude were vitiated by his ignorance of aberration, &c.; but, by a happy chance, these errors almost balanced each other. The extremities of his arc were Sourdon (near Amiens), and Malvoisine (near Paris). His base was 5663 toises; his base of verification 3902 toises. The difference of latitude of Malvoisine and Sourdon was found to be 1°11′54″; and when the measure was extended to Amiens, the difference of latitude of Malvoisine and Amiens was found to be 1°22′55″. The corresponding arcs were 68,430 and

78,850 toises; the first gave for the length of an arc of 1°, 57,064 toises, the second 57,057; the mean is 57,060 toises = 60,812 fathoms. The Astronomical observations were repeated in 1739 by Maupertuis, Clairaut, &c.: they found 1° = 57,183 toises.

In 1673 appeared the Work of Huygens, entitled "De Horologio Oscillatorio." In this, for the first time, were found correct notions on the subject of centrifugal force. It does not appear, however, that these were applied to the theoretical investigation of the Earth's form before the publication of Newton's "Principia."

In 1666, Newton appears to have first entertained the idea of Gravitation. It is remarkable that, at this time, he seems to have been unacquainted with the measures of Norwood and Snell; and considering the length of a degree, according to the usual estimation, to be sixty miles, he was induced, by the disagreement of his calculations from this estimation, to lay aside his theory of Gravitation. For the Moon's parallax being known, that is the proportion of the Earth's radius to the radius of the Moon's orbit (supposed a circle) being known, the force of Gravity on the Moon was found by Newton's law; and the periodic time was also known; and from these data, and the theorems of Huygens and Newton relating to motion in a circle, it was easy to calculate the radius of the circle in which it moved, and consequently (from the proportion above mentioned) the radius of the Earth: if this were not the same as the radius given by the measures, Newton's law could not be true. The measure of Picard enabled him to establish the theory; and it is this measure that is used in the "Principia" published in 1687. In this wonderful Work a prodigious step was made towards the theory of the Earth's form. Combining the theory of centrifugal forces with the properties of fluids, Newton showed that the Earth must be not spherical but spheroidal; that its equatorial diameter must be longer than its axis of revolution; and he actually calculated the proportion of the diameters, on the supposition that the Earth had been in the state of a homogeneous fluid, to be 229:230. With regard to this astonishing investigation we shall merely state, that though defective, it is not erroneous; it is one of the many instances in which Newton has obtained a correct result by means apparently quite inadequate. He showed, also, that Gravity must be less at the Equator than near the Poles; and this served to explain a very remarkable fact that had lately been observed. In 1671, Richer, who had been sent by the French Government to Cayenne

for the purpose of conducting a series of Astronomical observations, found that his clock, which had been regulated to mean time at Paris, lost more than two minutes every day. Similar facts were afterwards observed by Varin and Deshayes on the coasts of Africa and America; and in all cases the alteration of the clock's rate was much greater than any that could be caused by the change of temperature.

But the most remarkable of Newton's inferences is, perhaps, his explanation of the precession of the Equinoxes. This motion of the Earth's axis was discovered by Hipparchus (B.C. 150), and all succeeding Astronomers had recognised its existence. Eighteen centuries after its discovery, it was shown by Newton to be a consequence of the attraction of the Sun and Moon on the Earth's equatorial protuberance. Having shown that the attraction of a distant body on a satellite would produce a retrograde motion of its nodes, he observes that if we first suppose a number of satellites to revolve in one plane, and then suppose them to be connected in such a manner as to become a solid ring revolving in its plane, we shall still have a retrograde motion of the nodes: and if the Earth be fixed within the ring, the motion of the nodes, though less than before, will still be retrograde: and the transition from this case to that of an oblate spheroid is sufficiently obvious. The calculation which Newton has attempted to found on this reasoning is in some respects erroneous; but the explanation is, perhaps, one of the strongest proofs of genius. Indeed, if at this time we might presume to select the part of the "Principia" which probably astonished, and delighted, and satisfied its readers more than any other, we should fix without hesitation on the explanation of the Precession of the Equinoxes.

In 1690 was published Huygen's Treatise, "De Causâ Gravitatis." It contains an investigation of the figure of the Earth, supposing the attraction upon every particle to be directed towards the centre, and to be always the same at equal distances from the centre. This supposition, it will be remarked, is directly opposed to one part of the Principle of Gravitation, namely, that which states that every particle attracts every other particle. The ratio of the axes is found in this manner to be 578:579 . . .

In 1764, Messrs. Charles Mason and Jeremiah Dixon, who had before been engaged by the Royal Society for some important Astronomical observations, were employed in settling the boundaries of Maryland and Pennsylvania in North America. The line

which they traced out in the peninsula between Chesapeake Bay and Delaware Bay seemed so favourable for a meridian-measure, that, on their representation, the Council of the Royal Society furnished them with standards and instructions, and procured the loan of a sector belonging to Mr. Penn. An account of the measure is given in the *Phil. Trans.* for 1768. This measure differs from all others made since the time of Norwood in this respect—that no triangles were used, but the whole line (about 100 miles) was measured with rods. These rods were compared with a five-feet brass rod made by Bird. The whole length was found to be 538,-067 feet, and the difference of latitude 1°28′45″; whence 1° = 363,763 feet = 60,627⅙ fathoms. It had been found by a comparison of standards that the English fathom = $^{107}/_{114}$ × French toise; whence one degree was inferred to be 56,904½ toises. But after a new comparison of the toise and fathom, made under the inspection of Dr. Maskelyne, and after applying some small corrections, this was reduced to 60,625 fathoms, or 56,888 toises. The mean latitude was 39°12′ . . .

Earth's Ellipticity from the Motion of the Moon.—Nearly at the conclusion of this century the investigations of Laplace ("Mécanique Céleste") furnished us with a curious method of determining the ratio of the Earth's axes. He showed that, in consequence of the Earth's oblateness, the Moon's motion would not be the same as if the Earth were spherical; and that two of the resulting irregularities would rise to such a magnitude as probably to be sensible. From the accurate observations of the Moon, made at Greenwich, the existence of these inequalities has been detected, and their magnitude (about 8″ each) has been ascertained with tolerable certainty. They indicate an ellipticity of nearly $^{1}/_{300}$ . . .

Conclusion.—1. The measures of the Earth, the observations of pendulums, and the lunar inequalities, agree in showing that the Earth's form does not differ much from that of an ellipsoid of revolution whose ellipticity is (we think certainly) greater than $^{1}/_{300}$, and whose major semiaxis is about 20,923,700 English feet.

2. The phenomena of precession and nutation give an ellipticity rather smaller; but as no result can be deduced from them except on an assumed law of density, this value cannot be put in opposition to the others.

3. As the results of the pendulum observations, the lunar inequalities, and the precessional phenomena, can only be used to

determine the Earth's form by the intermediation of the principle of gravitation, the very near coincidence of the results is a strong argument in favour of the truth of that principle.

4. The same things make it highly probable that the Earth has once been in a fluid or semifluid state.

5. None of these results can be obtained without the admission of considerable anomalies, all of which, however, appear to be consistent with the principle of gravitation.

6. The mean density of the Earth considerably exceeds, and is probably double of the density of the superficial rocks.

7. The near agreement of the proportion between these as deduced from an assumed law with the proportion found by the experiments with leaden balls (where it is assumed in the calculation that the law of gravitation holds good at the distance of a few inches) makes it probable that the law is sensibly true to very small distances.

F. G. W. STRUVE[1]

Concerning the Parallax of the Fixed Stars

(From "Mensuræ Micrometricæ," 1837; translation by J. Jackson, *The Observatory*, 1922.)

Steinheil, from a comparison of the brightness of Arcturus and the Sun, concluded that if the Sun were removed to 3,286,500 times its present distance its brightness would be reduced to that of Arcturus. Hence it follows that the parallax π of Arcturus is given by

$$\sin \pi = \frac{1}{A \cdot 3286500} \text{ or } \pi = \frac{1}{A} \cdot 0''\cdot 0625,$$

where $A = d\sqrt{l}$ is the product of the diameter d of Arcturus by the square root of its intrinsic brilliance l, both expressed in terms of the same quantities for the Sun. If now we put $A = 1$, *i.e.*, if Arcturus and the Sun appear equally bright at the same distance

$$\pi = 0''\cdot 0625 = \tfrac{1}{16}'',$$

and the change in the direction of Arcturus, as seen from opposite sides of the orbit of the Earth, may rise to $\frac{1}{8}''$. Amongst a number of stars values of A less than 1 are not less to be expected than greater values. Whence we conclude that amongst stars of the first magnitude we may expect some parallaxes giving displacement exceeding $\frac{1''}{8}$, *i.e.*, large enough to give some hope of being detected by present-day methods. If, indeed, the parallax π of a star could be determined by observation and if s be its apparent brightness compared with that of the Sun, then the product $d^2 l = s/\sin \pi$ would be a known quantity. It would thus be possible to determine the intensity l of the brightness of a star as soon as d is found, *i.e.*, as soon as the apparent diameter of the star is

[1] Friedrich Georg Wilhelm Struve (1793–1864), Russian astronomer, was the first director of the Pulkova observatory. His contributions relate mainly to geodesy and to double-star astronomy. The above contribution shows a remarkably keen appreciation of the problems of estimating and measuring the distances of the stars.

measured. There is, however, no hope of observing the true angular diameter of a star, for the diameter of the Sun removed to 3,286,500 times its distance from us, at which distance it would

F. G. W. Struve.

appear like Arcturus, would not exceed $\frac{1}{1700}''$. The diameters shown by the most perfect telescopes are therefore spurious.

A second index to the distances of the fixed stars is found in their proper motions. Whatever may be the origin of these motions, the apparent proper motion is inversely proportional to

the distance of the star from the solar system. If 61 Cygni were transported to half its distance from the Sun, without change in its absolute motion, the apparent annual proper motion would be 10″·332 instead of 5″·166. It follows that those stars are to be judged nearest to us which have the largest proper motions.

A third criterion for comparing distances is presented by double stars. According to Sir John Herschel the apparent semimajor axis of 70 Ophiuchi is 4″·3. At half the distance it would appear as 8″·6. Those double stars, therefore, are to be considered nearer to us for which the angular separation is greater, unless they should be merely optical double stars. But, indeed, the linear dimensions are indicated by the periods of revolution. For if π is the parallax of a double star and M the combined mass of the stars in terms of the Sun's mass, and if a the semi-major axis of the true orbit expressed in seconds of arc and T the period of revolution are deduced from the observations, then

$$\pi = \frac{a}{T^2 M^{\frac{1}{3}}}.$$

If we apply this formula to the double stars, for which the true orbits are more accurately known and take the elements given by Sir John Herschel for 3 stars and by Maedler for σ Coronæ and ξ Ursæ Majoris we obtain

for γ Virginis	$\pi M^{\frac{1}{3}} = 0''\cdot185$,
for Castor	$\pi M^{\frac{1}{3}} = 0\ \cdot200$,
for σ Coronæ	$\pi M^{\frac{1}{3}} = 0\ \cdot085$,
for ξ Ursæ Majoris	$\pi M^{\frac{1}{3}} = 0\ \cdot147$,
for 70 Ophiuchi	$\pi M^{\frac{1}{3}} = 0\ \cdot236$,

and we may add if we estimate for ζ Herculis $a = 1''\,0$, $T = 14$, and for 61 Cygni $a = 16''$, $T = 500$,

for ζ Herculis	$\pi M^{\frac{1}{3}} = 0''\cdot172$,
for 61 Cygni	$\pi M^{\frac{1}{3}} = 0\ \cdot254$.

We see from these results that the parallax of double stars may reach $\frac{1}{4}''$ if their masses are not greater than that of the Sun. For 61 Cygni the parallax would be 0″·1 if the mass were 16.4. Those stars are well worth attention for which the proper motion and the values of $\pi M^{\frac{1}{3}}$ are largest—61 Cygni, 70 Ophiuchi, and ξ Ursæ Majoris. This method of estimating parallaxes, from the periods and semi-axes of the orbits of double stars is not less to be trusted than that based on comparisons of brightness. In the

one we assume equality in the brightness of the Sun and star at the same distance, in the other equality of the masses. Either hypothesis is to be considered equally probable, although, in reality, it may be far from the truth.

We conclude, therefore, that the probability of a small distance or large parallax is greatest for those stars for which different criteria are satisfied. Thus amongst the most brilliant stars we must consider those nearest which have the largest proper motions; and amongst stars with large proper motions the more brilliant and the doubles with rapid orbital motion are probably nearest. It is not without a cause that large proper motion is more common in bright stars than in others. Taken in the mean one star in six of those given in Bessel's "Fundamenta" has a motion exceeding 0″·2. Amongst the 13 stars of the first magnitude there are 8 with a proper motion greater than 0″·2—Capella, Sirius, Procyon, Regulus, Arcturus, α Lyræ, α Aquilæ, and α Piscis Australis. For 3 of these (Sirius, Arcturus, and Procyon) the annual motion even exceeds 1″, and these are the most brilliant stars seen from Europe. To them may be added α Lyræ with the smaller motion of 0″·36. From all these considerations we are able to indicate those stars, which before all others, offer the hope that their parallaxes may be detected:

(1) Stars of the first and second magnitude.

(2) Stars with proper motion exceeding 1″ annually—η Cassiopeiæ, μ Cassiopeiæ, τ Ceti, δ Trianguli, ι Persei, 40 Eridani, Sirius, Procyon, θ Ursæ Majoris, 61 Virginis, Arcturus, γ Serpentis, 72 Herculis, 70 Ophiuchi, σ Draconis, 61 Cygni, Argelander 540, 85 Pegasi.

(3) Double stars, which for their apparent separation show the most rapid orbital motion—61 Cygni, 70 Ophiuchi, η Cassiopeiæ, ξ Ursæ Majoris, Castor, γ Virginis, ζ Herculis, δ Herculis.[1]

We see that some stars are common to (1) and (2) and also to (2) and (3). To the third class 40 Eridani should be added for its association with a star of the ninth magnitude, 83″ distant, is a remarkable phenomenon. Amongst all the stars the following appear to be most worthy of being observed for parallax:

α Tauri, Capella, Sirius, Procyon, Arcturus, α Lyrae, α Aquilae, Castor, 61 Cygni, 40 Eridani, μ Cassiopeiæ, Argelander 540, 70 Ophiuchi, ξ Ursae Majoris, γ Virginis, and ζ Herculis.

[1] Struve believed δ Herculis to be a binary and not an optical double star. (*Note by translator.*)

On the Motion of Double Stars in Their Orbits

(Translated from "Mensuræ Micrometricæ" by Dr. John H. Walden, 1928.)

The physical connection in double stars we have so far deduced from two arguments, one of which was drawn from the slight probability of a purely optical connection, and the other from the proper motion common to the group. These arguments, although very strong, are nevertheless indirect. That two stars are united by attraction can be proved directly from the simple effect of the attraction itself, that is, from the fact that the relative motion of one star is in a curve concave to the other, often greater, star. This curve, if the attraction between the fixed stars follows the law of Newton, is a conic section. In this case, one star must be accepted as being in the focus of the conic section, around which focus, in accordance with the rules of Kepler, sectors proportional to the times will be described by the other star.

From these considerations it is now apparent how important is the inquiry into the motions of the double stars, which depends on continued observations. It will inform us whether the law of Newton, that attractions are inversely proportional to the squares of the distances—a law that every experiment and observation has shown to be true in the solar system—is the law of the entire stellar world.

We cannot, however, observe the actual curves of the motions of the double stars, but only their orthographic projections on a plane normal to the direction of the star as seen from the Earth. The problem of practical astronomy that is now set before us with reference to the double stars is to determine with the greatest care the apparent curves of the relative motion. If, after reasonably extended observations, we shall have found that the apparent orbits are elliptical, then we shall have to set up another inquiry, to determine whether, as more careful measurements are made, the sectors are projected exactly proportional to the times. When this has been proved, we shall have learned that the motions of the double stars take place according to the laws of Kepler, and we shall have a right to consider that the place which one star occupies within the apparent ellipse is the projection of the focus of the elliptical orbit itself. When this hypothesis has been accepted, then, from the dimensions of the apparent orbit and from the place of one star, the situation of the plane of the true orbit, and

the dimensions of the orbit, expressed in degrees, can be deduced without difficulty. If, furthermore, we shall succeed sometime in determining the parallax of a double star which has been thus examined, the dimensions of the true orbit expressed in radii of the terrestrial orbit, and the sum of the masses of the two stars as compared with the mass of the Sun, will become known. If, finally, we shall also be able to determine a point that is at rest in the system, that is, the position of the centre of gravity, we shall be able to determine the masses of the individual stars.

Very famous men—Savarius, Encke, the younger Herschel especially, and, finally, Mädler—have devoted time and effort to calculating from observations the true orbits of the double stars; and from their labors the approximate times of revolution and the other elements of certain systems—one true diameter alone being excepted because of a lacking parallax—are generally known. But in all these investigations I find it assumed that the motions take place according to the laws of Kepler, a fact which is first to be determined by the observations themselves. The material for the calculation is given by the measurements on double stars which that great investigator of the stellar world, the elder Herschel, made around the years 1780 and 1802, compared with the observations which in our times have been made, beginning with the year 1814, at Dorpat, London, Passy, Slough, Königsberg, Ormskerk, Berlin, and elsewhere. But that exactness of measurement which is necessary, if the inquiry into the elements of the orbits is to be carried through with some success, has not been attained until very recent times. That the most of the elder Herschel's angles are fairly certain cannot be doubted, although the larger part of them seem to have been deduced from observations made from day to day. But of far less certainty and value are the distances which are given in Herschel's catalogues. The elder Herschel has expressed nearly all the distances less than 4″, and most of those between 4″ and 8″, simply by their relation to the diameters of the stars as these diameters appeared in the telescope. Greater distances he gave by the arc. But since in the case of most of the stars simply the one distance is given, no judgment can be made about its worth from its agreement. This, indeed, seems certain: if we compare the distances which are greater than 32″ as measured by the elder Herschel with the most recent measurements, they are nearly all less than the present-day measurements. Whence it follows that Herschel's distances are full of serious errors, and the

suspicion is aroused that the smaller distances also, from 8″ to 32″, are subject to similar errors. It seems probable that the average errors of the Herschelian distances can in a manner be explained away by a complete comparison of these distances with the best calculations of the present day, since it is reasonable to suppose that on the average from a large number there will be practically no effect of variations. Since, as is clear from the measurements which the younger Herschel extracted from the records in the Herschelian manuscripts for certain stars, there should be many measurements which Herschel did not make public, it is very greatly to be desired that a new complete edition of the elder Herschel's micrometric measurements for double stars be prepared. This edition, if it presents the measurements in the original form, will provide a means of deducing with probability or certainty the errors of reading which are clearly present even in the case of some angles. These errors are evident from the fact that some angles given by the elder Herschel in no way agree with the relations which more recent measurements demand. There is another thing that we hope to have corrected in a new edition. The elder Herschel in the catalogues of the years 1782 and 1784 assigns a date to each star, which appears to be the date of the day on which he detected that the star was double. That the measurements, however, often fit another date is clear from the positions cited by the younger Herschel. May I be permitted to express here the hope that the younger Herschel, on his return from Africa, will again direct his attention to his father's measurements made on the double stars? . . .

The apparent proximity of two double stars depends on two conditions: on the true diameter and position of the orbit and on the distance of the stars from the Sun. Let us suppose that Castor, whose stars, of magnitude 2.7 and 3.6, are now distant from each other 4″·8, be removed to a position distant from the Sun six times as great as its present distance. We shall see that the double star is then to be ranked in the first order [instead of the fourth], its distance being 0″·8, and its two stars of magnitudes 7 and 7.5, or, even fainter, if extinction of light takes place. Stars, therefore, of the first order, if they had obtained their apparent proximity simply from their greater distance from the Sun, ought in general to be far less bright than stars of the other orders. This is not the case, however, since stars of the first order [distances 0″ to 1″] are just as bright as stars of the other orders.

For, taking the average of 55 pairs of the first order, I found the average magnitude of the star belonging to this order in the section of bright stars to be equal to 7. In the same way, of 49 pairs of bright stars of the eighth order [distances 24″ to 32″] I found the average magnitude to be 7. Not the slightest difference was apparent. If we join this argument with that most certain argument which was deduced above from the velocities of the motions in the orbits, the following very important theorem may be considered as absolutely proven: *The division of double stars into orders according to their mean distances from each other does not rest on a merely optical basis. In general, stars of the first order distant from each other between* 0″ *and* 1″ *are also those whose linear distances from each other are the least, and so on in the other orders. The nearer to each other, therefore, the stars appear to be, the greater in general is the action of gravity whereby they are connected, the more quickly they move in their orbits around the center of gravity, and the smaller are their periods of revolution.* It is easily understood that this statement is no more than general, and in particular cases admits exceptions.

BESSEL[1]

The Parallax of 61 Cygni

(From "A Letter to Sir J. Herschel, Bart.," dated Königsberg, Oct. 23, 1838, *Monthly Notices* of the Royal Astronomical Society, 1838.)

Esteemed Sir,—Having succeeded in obtaining a long-looked-for result, and presuming that it will interest so great and zealous an explorer of the heavens as yourself, I take the liberty of making a communication to you thereupon. Should you consider this communication of sufficient importance to lay before other friends of Astronomy, I not only have no objection, but request you to do so. With this view, I might have sent it to you through Mr. Baily; and I should have preferred this course, as it would have interfered less with the important affairs claiming your immediate attention on your return to England. But, to you, I can write in my own language, and thus secure my meaning from indistinctness.

After so many unsuccessful attempts to determine the parallax of a fixed star, I thought it worth while to try what might be accomplished by means of the accuracy which my great Fraunhofer Heliometer gives to the observations. I undertook to make this investigation upon the star 61 *Cygni*, which, by reason of its great proper motion, is perhaps the best of all; which affords the advantage of being a double star, and on that account may be observed with greater accuracy; and which is so near the pole that, with the exception of a small part of the year, it can always be observed at night at a sufficient distance from the horizon. I began the comparisons of this star in September, 1834, by measuring its distance from two small stars of the 11th magnitude, of which one precedes, and the other is to the northward. But I soon perceived that the atmosphere was seldom sufficiently favourable to allow of the observation of stars so small; and, therefore, I resolved to

[1] Friedrich Wilhelm Bessel (1784–1846), director of the observatory at Königsberg, obtained the first satisfactory measure of the parallax of a star; he also reduced Bradley's observations of stellar positions. From the irregularity in the motion of Sirius he predicted the faint companion, later discovered by Alvan G. Clark (1832–1897), the renowned American maker of great telescope lenses.

select brighter ones, although somewhat more distant. In the year 1835, researches on the length of the pendulum at Berlin took me away for three months from the observatory; and when I returned, Halley's Comet had made its appearance, and claimed all the clear nights. In 1836, I was too much occupied with the calculations of the measurement of a degree in this country, and with editing my work on the subject, to be able to prosecute the observations of α Cygni[1] so uninterruptedly as was necessary, in my opinion, in order that they might afford an unequivocal result. But, in 1837 these obstacles were removed, and I, thereupon, resumed the hope that I should be led to the same result which Struve grounded upon his observations of α *Lyræ*, by similar observations of 61 *Cygni*.

I selected among the small stars which surround that double star, two between the 9th and 10th magnitudes; of which one, *a*, is nearly perpendicular to the line of direction of the double star; the other, *b*, nearly in this direction. I have measured with the heliometer the distances of these stars from the point which bisects the distance between the two stars of 61 *Cygni;* as I considered this kind of observation the most correct that could be obtained, I have commonly repeated the observations sixteen times every night. When the atmosphere has been unusually unsteady, I have, however, made more numerous repetitions; although, by this, I fear the result has not attained that precision which it would have possessed by fewer observations on more favourable nights. This unsteadiness of the atmosphere is the great obstacle which attaches to all the more delicate astronomical observations. In an unfavourable climate we cannot avoid its prejudicial influence, unless by observing only on the finest nights; by which, however, it would become still more difficult to collect the number of observations necessary for an investigation . . .

The tables contain all my measures of distance, freed from the effects of refraction and aberration, and reduced to the beginning of 1838. In these reductions, the annual variations employed of both distances are $= +4''{\cdot}3915$ and $-2''{\cdot}825$; which I have deduced (on the supposition that the stars *a* and *b* have no proper motions) from the mean motions of both stars of 61 *Cygni*, which M. Argelander had lately found by comparison of my determination (from Bradley's observations) for 1755, with his own for 1830. In the meantime, we cannot regard these variations of distance

[1 Probably 61 Cygni is meant.]

as the *true variations;* because the stars compared may have proper motions, and, also, because it is not known whether the mean of the motions of both stars of 61 *Cygni* appertains to its centre, and whether *this* (motion) is proportional to the time. In what follows, let us denote the true variations of the distances by $+4''{\cdot}3915 + \alpha'$ and $-2''{\cdot}825 + \beta'$, the mean distances for the beginning of 1835 by α and β; the time, reckoned from this beginning, by t; the difference of the constants of the annual parallax of 61 *Cygni*, and of the comparison-stars a and b, by α'' and β''; and, lastly, the coefficients of the parallax depending on the place of the earth by a. Then the expressions of the distances at the beginning of 1838 are—

For the star $a = \alpha + t\alpha' + a\alpha''$

For the star $b = \beta + t\beta' + a\beta''$ . . .

I have employed the preceding list of the observations of the distances of the star 61 *Cygni* from a and b, in two different ways, in order to deduce from it results for the annual parallax of α *Cygni*. I have first assumed α'' and β'' as independent of each other; or, in other words, considered it as not improbable that a and b themselves may possess sensible parallax. In this way I have found,

			Mean error
	For the star a		
Mean distance for the beginning of 1838....		461″·6094	
Annual variation = $+4''{\cdot}3915 - 0''{\cdot}0543$...		+4 ·3372	±0″·0398
Difference of annual parallax of 61 and a...	$\alpha'' = +0$	·3690	±0 ·0283
	For the star b		
Mean distance for the beginning of 1838....		706·2909	
Annual variation = $-2''{\cdot}825 + 0''{\cdot}2426$....		−2·5824	±0 ·0434
Difference of annual parallax of 61 and b....	$\beta'' = +0$	·2605	±0 ·0278

The observations seem also to indicate that the difference of the parallaxes of 61 and b is smaller than that of 61 and a; which must be the case, indeed, if b itself have a sensible parallax greater than a. The difference of the computed values of α'' and β'', in fact, exceeds the limits of the probable uncertainty of the observations; but it is to be observed that the probability of *equal* values of α'' and β'' is not so small that we should be inclined to consider the difference of the two as *proved* by the observations. Further observations will increase the weight of both results, and, at the same time, give more accurate values of the annual variations.

I have, therefore, deduced a second result from the observations,

which rests on the supposition that the parallaxes of *a* and *b* are *insensible;* or that α'' and β'' are equal. For this purpose, since both series must now be brought into connexion with one another, it was necessary to deduce the *weight* of the observations contained in the second series, the weight of those in the first series being taken as unit. I have found it = 0·6889; and hence the most probable value of the annual parallax[1] of 61 *Cygni* = 0″·3136. On this hypothesis, I find the mean distances of both stars for the beginning of 1838, to be 461″·6171 and 706″·2791; and the corrections of the assumed values of the annual variations, = −0″·0293 and +0″·2395. The mean error of an observation of the kind of which I have assumed the weight as unit, is ±0″·1354, and the mean error of the annual parallax of 61 *Cygni*, = ±0″·0202 . . .

As the mean error of the annual parallax of 61 Cygni (= 0″·3136) is only ±0″·0202, and consequently not $\frac{1}{15}$ of its value computed; and these comparisons show that the progress of the influence of the parallax, which the observations indicate, follows the theory as nearly as can be expected considering its smallness, we can no longer doubt that this parallax is sensible. Assuming it 0″·3136, we find the distance of the star 61 Cygni from the sun 657,700 mean distances of the earth from the sun: light employs 10.3 years to traverse this distance. As the annual proper motion of α Cygni amounts to 5″·123 of a great circle, the *relative* motion of this star and the sun must be considerably more than sixteen semidiameters of the earth's orbit, and the star must have a constant aberration of more than 52″. When we shall have succeeded in determining the elements of the motion of both the stars forming the double star, round their common centre of gravity, we shall be able also to determine the sum of their masses. I have attentively considered the preceding observations of the relative positions; but I consider them as yet very inadequate to afford the elements of the orbit. I consider them sufficient only to show that the annual angular motion is somewhere about $\frac{2}{3}$ of a degree; and that the distance, at the beginning of this century, had a minimum of about 15″. We are enabled hence to conclude that the time of a revolution is more than 540 years,[2] and that the semimajor axis of the orbit is seen under an angle of more than 15″. If, however, we proceed from these numbers, which are merely *limits*, we find the

[1 The value now accepted is 0″·300 ± 0″·005, p.e.]

[2 Subsequent measures of relative motions indicate that the period of revolution is probably many thousands of years.]

sum of the masses of both stars less than half the sun's mass. But this point, which is deserving of attention, cannot be established until the observations shall be sufficient to determine the elements accurately. When long-continued observations of the places which the double star occupies amongst the small stars which surround it, shall have led to the knowledge of its centre of gravity, we shall be enabled to determine the two masses separately. But we cannot anticipate the time of these further researches.

SCHWABE[1]

The Periodicity of Sun Spots

(From *Astronomische Nachrichten*, Vol. 21, 1844.)

From my earlier observations, which I have communicated annually to this journal, there has already appeared a certain periodicity of sun spots, and the probable periodicity increases in certainty with this year's contribution. Although I indicated in Vol. 15, No. 350, p. 246 of the *Astronomische Nachrichten*, the number of groups for the years 1826–1827, nevertheless I include here a complete list of all sun spots observed by me, noting in addition to the number of sun spots also the number of days of observation and the number of days on which there were no spots.

Year	Groups	Number of days of no spots	Number of days of observation
1826	118	22	277
1827	161	2	273
1828	225	0	282
1829	199	0	244
1830	190	1	217
1831	149	3	239
1832	84	49	270
1833	33	139	267
1834	51	120	273
1835	173	18	244
1836	272	0	200
1837	333	0	168
1838	282	0	202
1839	162	0	205
1840	152	3	263
1841	102	15	283
1842	68	64	307
1843	34	149	324

[1] Samuel Heinrich Schwabe (1789–1875), German astronomer, through long series of observations established the law of the variations in the frequency of sun spots.

For the number of groups does not alone give sufficient accuracy for the determination of a period, because I am convinced that at times of great frequency of sun spots the number of groups is reckoned somewhat too small, while at times of their infrequent appearance the number is judged too large. In the first case several groups often merge into a single one, and in the second case it easily happens that one group, due to the disintegration of some spots, divides into two distinct groups. For this reason I shall probably be excused for repeating the former list.

If we compare the number of groups with the number of days free from spots, we find that the sun spots had a period of about ten years, and that throughout five years they appeared so frequently that during this time there occurred few, if any, days free from spots.

The future must decide whether this period shows constancy; whether the time of least activity of the sun in producing sun spots lasts one or two years, and whether this activity increases more rapidly than it decreases.

HUMBOLDT[1]

Shooting Stars

(From "Cosmos," Vol. 4, 1858; translated by E. C. Otté.)

Commencing from the *geometrical* relations of the periodic (not sporadic) falling stars, we direct our attention especially to what recent observations as to the *divergence* or *point of departure* of the meteors, and their *entirely planetary velocity*, have made known. Both these circumstances, divergence and velocity, characterize them with a high degree of probability as luminous bodies which present themselves independently of the Earth's rotation, and penetrate into our atmosphere *from without*, from space. The North American observations of the *November period* on the occasion of the falls of stars in 1833, 1834, and 1837, indicated as the point of departure the star γ Leonis; the observations of the August phenomenon, in the year 1839, Algol in Perseus, or a point between Perseus and Taurus. These centers of divergence were about the constellations toward which the Earth moved at the same epoch. Saigey, who has submitted the American observations of 1833 to a very accurate investigation, remarks that the fixed radiation from the constellation Leo is only observed properly after midnight, in the last three or four hours before daybreak; that of eighteen observers between the town of Mexico and Lake Huron, only ten perceived the same general point of departure of the meteors, which Denison Olmstead, Professor of Mathematics in New Haven (Connecticut), indicated.

The excellent work of Edward Heis, of Aix-la-Chapelle, which presents in a condensed form the very accurate observations of falling stars made by himself during ten years, contains results as to the *phenomena of divergence*, which are so much the more important as the observer has discussed them with mathematical strictness. According to him, " . . . the falling stars of the *November period* present the peculiarity that their paths are more

[1] Alexander von Humboldt (1769–1859), German naturalist and traveler, laid the foundation of the sciences of physical geography and meteorology in their larger bearings.

dispersed than those of the *August period.* In each of the two periods there were simultaneously several points of departure by no means always proceeding *from the same constellation,* as there was too great a tendency to assume since the year 1833." Besides the *principal point of departure of Algol in Perseus,* Heis finds in the *August* periods of the years 1839, 1841, 1842, 1843, 1844, 1847; and 1848, two others in Draco and the *North Pole.* "In order to deduce accurate results as to the points of departure of the paths of the falling stars in the *November periods* for the years 1839, 1841, 1846, and 1847, for the four points (Perseus, Leo, Cassiopeia, and the Dragon's Head), the mean path belonging to each was drawn upon a thirty-inch celestial globe, and in every case the position of the point ascertained from which the greatest number of paths proceeded. The investigation showed that of 407 of the falling stars indicated *according to their paths,* 171 came from Perseus, near the star η in Medusa's Head, 83 from Leo, 35 from Cassiopeia, near the changeable star α, 10 from the Dragon's Head but full 78 from undetermined points. The number of falling stars issuing from Perseus consequently amounted to nearly double those from Leo."

The divergence from Perseus has consequently shown itself *in both* periods as a very remarkable result. An acute observer, Julius Schmidt, attached to the Observatory at Bonn, who has been occupied with meteoric phenomena for eight or ten years, expresses himself upon this subject with great decision in a letter to me (July, 1851): "If I deduct from the abundant falls of shooting stars in November, 1833, and 1834, as well as from subsequent ones, that kind in which the point in Leo sent out whole swarms of meteors, I am at present inclined to consider the *Perseus point* as that point of divergence which presents not only in August, but throughout the *whole year,* the most meteors. This point is situated, according to the result deduced from 478 observations by Heis, in Rt. Asc. 50.3° and Decl. 51.5° (holding good for 1844–1846). In November, 1849 (from the 7th to the 14th), I saw some hundreds more shooting stars than I have ever remarked since 1841. Of these only a few, upon the whole, came from Leo; by far the greater number belonged to the constellation of Perseus. It follows from this, as it appears to me, that the *great* November phenomenon of 1799 and 1833 did not appear at that time (1841). Olbers also believes that the maximum November appearance has a period of thirty-four years. If the directions of the meteor-

paths are considered in their full complication and periodical recurrence, it is found that there are certain *points of divergence* which are always represented, others which appear only sporadically and changeably."

Whether, moreover, the different points of divergence alter with the years—which, if *closed rings* are assumed, would indicate an alteration in the situation of the ring in which the meteors move—can not at present be determined with certainty from the observations. A beautiful series of such observations by Houzeau (during the years 1839 and 1842) appears to offer evidence against a progressive alteration. Edward Heis has very correctly remarked that, in Grecian and Roman antiquity, attention had already been directed to a certain temporary uniformity in the *direction* of shooting stars darting across the sky. That direction was then considered as the result of a wind already blowing in the higher regions of the atmosphere, and predicted to the sailors an approaching current of air descending thence into the lower regions.

If the *periodic* streams of shooting stars are distinguished from the *sporadic* by the frequent parallelism of their paths, proceeding from one or more points of divergence, a second criterion of them is the numerical—the number of individual meteors referred to a definite measure of time. We come here to the much-disputed question of the distinction of an extraordinary from an ordinary fall of shooting stars. Two excellent observers, Olbers and Quetelet, have given as the mean number of meteors which can be reckoned hourly in the range of vision of one person upon not extraordinary days, the former five to six, the latter eight meteors. For the discussion of this question, which is as important as the determination of the laws of motion of shooting stars, in reference to their direction, a great number of observations are required. I have therefore referred with confidence to the already mentioned observer, Herr Julius Schmidt at Bonn, who, long accustomed to astronomical accuracy, takes up with his peculiar energy the whole phenomena of meteors—of which the formation of aërolites and their fall to the Earth appear to him merely a special phase, the rarest, and, therefore, not the most important. The following are the principal results of the communications which I requested from him.

"The mean number of *sporadic* shooting stars appearing there has been found, from many years of observation (between 3 and 8 years), *a fall of from four to five in the hour.* This is the ordinary

condition when nothing periodic occurs. The mean numbers of *sporadic* meteors in the individual months give for the hour, January, 3.4; February, —; March 4.9; April, 2.4; May, 3.9; June, 5.3; July, 4.5; August, 5.3; September, 4.7; October, 4.5; November, 5.3; December, 4.0.

"Of the *periodic* meteors there may be *expected*, on the average, in each hour, *above* 13 *or* 15. For a single period, that of August, the stream of Laurentius[1] presented the following gradual increases from sporadic to periodic, upon an average of from three to eight years of observation.

Time	Number of meteors in one hour	Number of years
6th of August.....	6	1
7th of August.....	11	3
8th of August.....	15	4
9th of August.....	29	8
10th of August.....	31	6
11th of August.....	19	5
12th of August.....	7	3

. . . All these numbers refer to the circle of vision of one observer. Since the year 1838, the November falls have been less brilliant. (On the 12th of November, 1839, Heis still counted hourly 22 to 35 meteors; likewise, on the 13th of November, 1846, upon the average, 27 to 33.) So variable is the abundance of the periodic streams in individual years; but the number of the falling meteors always remains considerably greater than in ordinary nights, which show in one hour only four or five sporadic falls. The meteors appear to be the most seldom in January (calculating from the 4th), February, and March . . .

"The *upper* limits of the *height* of shooting stars can not be ascertained with accuracy, and Olbers considers all heights above 120 miles as being less certainly determined. The *lower* boundaries which were formerly generally estimated at 16 miles (over 97,388 feet), must be greatly contracted. Some, according to measurement, descend very nearly to the level of the summit of Chimborazo and Aconcagua, to the distance of four geographical miles above

[1 The Perseids, occurring about the tenth of August, the festival of St. Lawrence.]

the level of the sea. Heis remarked, on the contrary, a falling star seen simultaneously at Berlin and Breslau on the 10th of July, 1837, [which] had, according to accurate calculation, a height of 248 miles when its light first became visible, and a height of 168 on its disappearance; others disappeared during the same night at a height of 56 miles. From the older labors of Brandes (1823), it follows that of 100 well defined shooting stars seen from two points of observation, 4 had an elevation of only 4 to 12 miles; 15 between 12 and 24 m.; 22 from 24 to 40 m.; 35 (nearly one third) from 40 to 60 m.; 13 from 40 to 80 m.; and only 11 (scarcely one tenth) above 80 m., their heights being between 180 and 240 miles. From 4000 observations collected during nine years, it has been inferred, with regard to the *color* of the shooting stars, that two thirds are white, one seventh yellow, one seventeenth yellowish red, and only one thirty-seventh green."

Olbers reports, that during the fall of meteors in the night of the 12th and 13th of November, in the year 1838, a beautiful northern light was visible at Bremen, which colored large parts of the sky with an intense blood-red light. The shooting stars darting across this region maintained their white color unaltered, whence it may be inferred that the northern light was further removed from the surface of the Earth than the shooting stars were at that point where they became visible. The relative velocity of shooting stars has hitherto been estimated at from 18 to 36 geographical miles a second, while the Earth has only a translatory velocity of 16.4 miles. Corresponding observations of Julius Schmidt at Bonn, and Heis at Aix-la-Chapelle (1849), gave as the actual minimum for a shooting star, which stood 48 miles vertically above St. Goar, and shot over the Lake of Laach, only 14 miles. According to other comparisons of the same observer, and of Houzeau in Mons, the velocity of four shooting stars was found to be between 46 and 95 miles in the second, consequently two to five times as great as the planetary velocity of the Earth. The cosmical origin is indeed most strongly proved by this result, together with the constancy of the simple or multiple points of divergence, *i.e.*, together with the circumstance that periodic shooting stars, independently of the rotation of the Earth, proceed during several hours from the same star, even when this star is not that toward which the Earth is moving at the same time. According to the existing measurements, fire-balls appear to move slower than shooting stars; but it nevertheless remains

striking, that when the former meteors fall, they sink such a little way into the ground. The mass at Ensisheim, in Alsace, weighing 276 pounds (November 7th, 1492), penetrated only 3 feet, and the aërolite of Braunau (July 14th, 1847) to the same depth. I know of only two meteoric stones which have plowed up the loose earth for 6 and 18 feet: these are the aërolites of Castrovillari, in the Abruzzi (February 9th, 1583), and that of Hradschina, in the Agram district (May 6th, 1751).

ARGELANDER[1]

Problems of Variable Stars and the Argelander Method

(From Schumacher's "Astronomisches Jahrbuch," 1844; translated by Annie J. Cannon, *Popular Astronomy*, 1912.)

Introduction.—The Greek and Roman writers and the Chronicles of the Middle Ages often mention new stars bursting forth, and it was sometimes added that after they shone for a period, they again became invisible. The most marvelous of these stars was the one which appeared in Cassiopeia, the last of October or the first of November, 1572, suddenly before the eyes of the astonished astronomers. It was as bright as Venus at its first appearance, but gradually became fainter, and disappeared in March, 1574. If we were inclined to regard the tales of the chroniclers of new stars as fables or errors, this was an apparition which proved the doubtful fact. And not alone for the contemporaries was this demonstrated; for so many precise descriptions and observations were gathered together in a Memoir by the distinguished astronomer, Tycho Brahe, the careful and expert observer of this phenomenon, that this remarkable appearance stands forth as an undisputed fact.

For a similar phenomenon did David Fabricius, pastor in Osteel in Ostfriesland, and previously pupil and assistant of Tycho's, consider a star of the third magnitude, which he saw in the early morning of August 13, 1596, one which he had never seen before, which he could not find in any catalogue, and which he could not again see after October of that year. When the star became very faint, and at last completely disappeared, Fabricius probably thought he had observed a new star. It is especially inconceivable why he did not make known this important observation sooner, why, above all, it aroused almost no sensation, and we find the first scanty mention of it eight years

[1] Friedrich Wilhelm August Argelander (1799–1875), German astronomer, published the great star catalogue of the northern hemisphere, the "Bonner Durchmusterung." Other important contributions are his essay on the movement of the sun among the stars and his work on variable stars.

later in two letters of Kepler. It was soon completely forgotten, and when Phocylides Holwarda, Professor in Franecker, in December of the year 1638, while observing a total eclipse of the moon, became aware of a new star in the breast of the whale, saw it increase to the third magnitude, then in January of the following year saw it decreasing, could barely glimpse it in the next August, but in December could plainly see it again, and made his observations known in print—then they remembered the star of Fabricius, and they found that both stood in one and the same place in the heavens, also the same place in which Bayer in his chart gave a star, but only of the fourth magnitude, which he designated by the letter *o*. So it was first known that a star became visible from time to time, and even in variable magnitude, between which time, it was invisible, not only to the naked eye, but also in telescopes. Notwithstanding the fact, that these appearances were so remarkable, yet they were observed only very incompletely by Fullenius, a colleague of Holwarda, by Jung in Hamburg, and Hevelius in Danzig. In 1660, however, Hevelius and, after him, some French astronomers, began to follow the star industriously. Bouillaud first noticed that the light changes of this wonderful star, which Hevel called Mira, had a somewhat regular period, and that within about eleven months, the star gradually decreases from its greatest brightness, remains invisible several months, and then anew gradually reaches its greatest brightness . . .

First about the end of the past century, there arose again somewhat more interest upon this subject, and there were two English astronomers, John Goodricke and Edward Pigott, who not only carefully observed the known variables, discovered the periodicity of Algol, and determined that of the others more precisely, but also greatly increased their numbers, by finding η Aquilæ, β Lyræ, δ Cephei, a small star in the northern crown (R Coronæ Borealis) and a similar one in Sobieski's Shield, (R Scuti) to be periodically variable. Then the elder Herschel appeared, who discovered a periodic variability in α Herculis, also Koch in Danzig, who did the same for a star in the Lion, No. 420 in Mayer's Catalogue (R Leonis). Later, Harding discovered new variable stars in Virgo, in Aquarius, and in the serpent of Ophiuchus. Bode, Olbers, and especially Wurm, busied themselves with this class of phenomena . . .

The Problems of Variable Stars.—Why do we attempt to explain the nature of these mysterious appearances, while they themselves

are yet so little known to us? Would we obtain a true and correct insight into these causes, should we wish to investigate thoroughly what these great forces are which from immeasurable distance give us knowledge of themselves in so remarkable a manner, or, if I may so express myself, to learn to comprehend the language in which these bodies, separated from us by endless space, communicate with us, and to understand the laws that govern them, then we must first learn to read the letters, must know and understand the words in which they speak to us. We must next study the appearances themselves more closely, must take pains to extricate the common properties and regularities, then we must seek the manner in which the departures from the rule occur, in order to know whether they follow each other in regular order or whether all order disappears, at least as far as is recognizable by us. We must, therefore, direct our attention to the following points:

1. The basis of the whole investigation of the variable stars is the length of the period. Without its precise knowledge, no binding together of the various observations is possible; the ascertainment of the greatest and least light or also any other phases, if these are constant, in epochs removed as far as possible from each other, must, therefore, go before all other investigations.

2. Of next importance is the investigation whether the period is always of equal length, or whether the differences between the times of maxima and minima, derived by means of the period, and those observed directly, are so great that we can no longer consider them to be errors of observation alone, but must seek their cause in irregularities of the appearances themselves. If these differences are great, then the decision of the question will not be difficult; where they are small, however, and especially in the case of the rapid light changes of those stars which pass through all their phases in a few days, this decision can be reached only through the coöperation of as many observers as possible, in order to eradicate the errors of each by the mean of many observations.

3. If now, we find such deviations from the mean period, it must be learned whether they are in any way proportional to the time, or whether we can possibly find another formula, dependent upon higher powers of the time, which will bring them in agreement with the observations, or at last, whether they are completely irregular, or perhaps only so complicated that we can discover no regularity therein.

4. Likewise, it is to be learned, whether the magnitudes which they attain at different maxima and minima, are always the same, at least within the errors of observation, or different at different times.

5. If such differences appear, they can in doubtful cases be ascertained only through the coöperation of many; it will then be our duty to discover whether they follow a fixed law, or are wholly without order, and in the first case, whether it is possible to find any relation between these laws and those which the variations in the period obey.

6. After, in this manner, the principal facts of the light changes are known, we can proceed to the observations lying between the maxima and minima. If we consider as abscissas, the times elapsed since the last maximum or minimum computed according to our formula, and as ordinates the magnitudes which correspond to these times according to the observations, we can draw a curve through the plotted points, called the Light Curve, which will give us the brightness for each intervening time. For each of the stars of long period, this light curve can be derived with some certainty, and even if we have found no formula for the irregularities, we may be able to derive it by comparing the differences in time between the dates given by the curve and the maximum and minimum observed directly. For the variables of short period, however, which pass through all their phases in a few days, if there are not at hand very diversified observations by many observers, we can attain accuracy only by the bringing together of the observations during many periods.

7. We must now seek, whether the light curve is the same in all periods, or whether also there are deviations here, whether these depend alone on the length of the period or the brightness which the star reaches in maximum, or whether they are more or less independent, or, above all, whether they follow fixed laws, or appear wholly irregular. Lastly we must turn our attention to still another point.

8. It has been remarked that the variable stars generally appear redder and less brilliant when decreasing than when increasing. A confirmation of this observation, which indeed, seems very problematical to me, and in which the fancy, or the notion formerly entertained, of a gradual conflagration, may have much share, is very important. This is something which those having sharp eyes, sensitive to color, should especially lay to heart.

If, by an investigation of all these relations, the rules should be found which govern these changes, and if our knowledge should be so precise that we can predict the magnitude any of these stars will have at any given time as exactly as a mean from a number of observations will determine it, only then shall we be able to say that we understand the appearances of variable stars, only then may we be able to hope a successful issue from our efforts to learn the laws which govern these apparitions . . .

Oh that a Copernicus or a Kepler might now arise for the investigation of the variable stars! Or that a Newton might appear who could unravel the complicated apparitions into the simplicity which always characterizes nature! We can not expect to accomplish such hopes in a moment, for we have just begun our investigation. It would indeed be audacious to indulge too freely in these hopes, after we have reminded ourselves of the manifold difficulties which stand in the way of their fulfilment; but I bespeak it in the strength of the manly spirit which generally rises in godlike power for the seeking out of truth and the Eternals—I bespeak it with faith in the striving for knowledge, which in our time pervades all branches of learning—I bespeak it to the encouragement of those who shrink back before great difficulties, who might despair at the view of the extensive field which is to be surveyed in order to reach the goal.

The Step-method of Observing Variables.—First, let suitable comparison stars be selected for each variable, consideration being given to the method of estimating previously indicated. They must be so chosen that one of them is fainter than the variable in its least light, if this is indeed to be observed, and another is brighter than the variable in its greatest light, the others so to follow each other in brightness that at least comparison stars are at hand for every five grades. For those variables now known I will give later under each, the comparison stars used by me.

Let as many of the comparison stars be used at every observation as one is able, without estimating too great intervals, but at least always one brighter and one fainter, also one other, if the variable appears completely or very nearly equal to one; the other comparisons will then serve to show the degree of the acquired precision, and if they are not very discordant, they will also make the result surer. Especially, whenever practicable, comparisons should be made with the mean between two stars, or rather it should be observed, how much nearer the variable comes to the

brighter or fainter, than to the other. If these two comparison stars are not too far apart in brightness, and if the variable stands nearly in the middle between them, such comparisons are very certain and show differences which, in all probability, would formerly have been overlooked. I denote such comparisons by putting the designations of both comparison stars beside each other within brackets, and the designation of the variable with the estimate of grades outside of the brackets, either before or after the same, according to whether the variable is brighter or fainter than the mean between the two stars. It is, however, not the difference from this mean which this number indicates, but the approximate number of grades by which the difference between the variable and one of the stars exceeds that between the variable and the other. For example, if the variable, *v*, is about two grades nearer to the bright star *a*, than to the faint star *b*, that is, if it is one step brighter than the mean between *a* and *b*, then I write *v*2(*ab*); if the variable is about one grade nearer to the fainter star, *b*, or about one half grade fainter than the mean, then I write (*ab*)1*v*.

As to the knowledge of the time, it is necessary to one or two minutes only in the case of Algol, for the others, it is accurate enough to the nearest hour; but the three stars of short period at times change their brightness with an hour about one quarter to one third of a magnitude, and therefore, in order not to magnify the error of the observation by an indeterminate time statement, something more precise is desirable. I generally give the time of the beginning and ending of the observations, and divide the intermediate in tenths of an hour by rough estimates under the several variables.

I have so arranged the reduction of the observations that for each moment of observation, the light value of the variable results in an absolute measure, expressed in grades over an arbitrary one, each variable star having a different zero point. First of all, the determination of the brightness of the comparison stars is necessary with reference to this zero point, or rather, with reference to the faintest of the comparison stars, which, if we desire, can be assumed at the zero point. For this determination, I use the collected observations of the variable, when it was compared with two other stars. I consider the sum of the grades, by which the variable is estimated to be brighter than one star and fainter than another, as the value of the difference in grades of the two comparison stars,

and then I take the mean of all the grade differences so derived. Only in a few cases, and when the comparison stars are of nearly equal light, do I ascertain their difference in brightness also from this difference. The differences obtained in this manner between any two of the comparison stars, I then combine with another, and thereby obtain the brightness of each with respect to the zero point. I take this occasion to observe that the fundamental unit of the grade is not the same for all the variables, but should not be very different.

After once the brightness of the comparison stars is found in numbers, then that of the variable can also be likewise expressed. But since, as a rule, more stars for comparison will be used in every observation, therefore one obtains more estimates for each one. It were now decidedly wrong if we take exactly the mean from these various determinations, because, as earlier expressed, the smaller differences can be more accurately estimated than the larger, and also, because the grades do not always have the same weight. One can obviate this influence by assigning weights to the several determinations in inverse ratio to the fundamental grade differences. Whereas, however, the errors in the estimates themselves are certainly not smaller than those in the grade assumptions, and since, furthermore, the first named uncertainty may not be exactly proportional to the difference; therefore, this method of reckoning would not give the truth and I take the mean from the results derived by both methods. But this is done rather by a survey than by computation; scrupulous precision being out of place here, because both results at most only differ from each other by a couple of tenths of a grade, quantities which have never been guaranteed.

I have now used the mean differences in brightness obtained from the various observations of the same pair of stars, in order to ascertain the accuracy of the estimates, and in the beginning, I found the unexpected result that the probable uncertainty in the comparison of two stars with each other is one half grade, or about the twentieth part of a magnitude. From 373 comparisons of two stars used for the variables δ Cephei, β Lyræ, η Aquilæ, and o Ceti, I find that the sum of the squares of the residuals of the several determinations = 347.59, and therefore, the mean error of any one such determination = 0.998, the probable error = 0.673. But since the intervals between the comparison stars are derived from the sum of the differences between the variable and

each star, two errors are here combined, that arising by the comparison of the variable with one star, and by the comparison with the other; it must, therefore, according to the theory of probability be divided by the square root of 2, if we wish to obtain the probable error of a comparison between two stars, and therefore will = 0.476 grades. Now this result can only be regarded as completely independent and sure, if the magnitudes of the stars are entirely unknown at the time of observing the several differences. Every knowledge of the same must influence the unity of the grade, because one would consciously or unconsciously be influenced by the known differences. For this reason, I consider that the probable error is perhaps too favorable; for although I derived it by using only those observations which I had made before the numerical determination of the differences between the various comparison stars, yet nevertheless even the approximate knowledge of these differences from the earlier observations must have had some influence on the later. But were this influence considerable, it would surely manifest itself, and very great errors would be avoided. This is not, however, the case, but errors occur, on the contrary, in greater numbers than they should according to the theory . . .

I am speaking so much at length upon this point because I wish to furnish as undeniable a proof as possible that the observations of variables with naked eye and without photometric instruments, give results which deserve confidence, and which are well adapted for the ascertainment of the peculiarities of these stars. But when we consider that in the case of many of these stars the whole variation, which is sometimes passed through twice in a few days, amounts to hardly 4 grades, and for others does not surpass 8 or 10 grades, so we see that in relation to this small variation, the probable error of any observation is always very important and that only its reduction by means of the multiplication of estimates can lead to a precision, which in some measure comes near to that attainable in other branches of astronomy. One or two observers are not adequate for this purpose; should the observations be repeated every hour or half hour, they would have no very great precision, for the earlier observations would always influence the later, and no independent, therefore no sure, result would be obtained. Especially are more observers necessary for the variables of short period, so that we may compute their light curve independently for each period, and recognize their irregularities

by comparison of the different curves. Could we be aided in this matter by the coöperation of a goodly number of amateurs, we would perhaps in a few years be able to discover laws in these apparent irregularities, and then in a short time accomplish more than in all the 60 years which have passed since their discovery.

Therefore do I lay these hitherto sorely neglected variables most pressingly on the heart of all lovers of the starry heavens. May you become so grateful for the pleasure which has so often rewarded your looking upward, which has constantly been offered you anew, that you will contribute your little mite towards the more exact knowledge of these stars! May you increase your enjoyment by combining the useful and the pleasant, while you perform an important part towards the increase of human knowledge, and help to investigate the eternal laws which announce in endless distance the almightly power and wisdom of the Creator! Let no one, who feels the desire and the strength to reach this goal, be deterred by the words of this paper. The observations may seem long and difficult on paper, but are in execution very simple, and may be so modified by each one's individuality as to become his own, and will become so bound up with his own experiences that, unconsciously as it were, they will soon be as essentials. As elsewhere, so the old saying holds here, "Well begun is half done," and I am thoroughly convinced that whoever carries on these observations for a few weeks, will find so much interest therein that he will never cease. I have one request, which is this, that the observations shall be made known each year. Observations buried in a desk are no observations. Should they be entrusted to me for reduction, or even for publication, I will undertake it with joy and thanks, and will also answer all questions with care and with the greatest pleasure.

SIR JOHN HERSCHEL[1]

Descriptions of η Argus and the Magellanic Clouds

(From "Results of Observations at the Cape of Good Hope, 1834–1838.")

There is perhaps no other sidereal object which unites more points of interest than this [η Argus]. Its situation is very remarkable, being in the midst of one of those rich and brilliant masses, a succession of which curiously contrasted with dark adjacent spaces (called by the old navigators coal-sacks), constitute the milky way in that portion of its course which lies between the Centaur and the main body of Argo. In all this region the stars of the milky way are well separated, and, except within the limits of the nebula, on a perfectly dark ground, and on an average, of larger magnitudes than in most other regions . . .

In the midst of this vast stratum of stars occurs the bright star η Argus, an object in itself of no ordinary interest on account of the singular changes its lustre has undergone within the period of authentic astronomy. For while in Halley's "Catalogue" (constructed in 1677) which is the first which can be entirely depended upon, it is marked as of the 4th magnitude, yet in Lacaille's and the subsequent Catalogues of Brisbane, Johnson, Fallows, and Taylor, it is made to rank as of the second. When first observed by myself in 1834, it appeared as a very large star of the second magnitude, or a very small one of the first, and so it remained without apparent increase or change up to nearly the end of 1837, in November of which year it was noticed of its usual brightness, or at least without exciting any suspicion of a change. Nor had any such suspicion been excited during a series of photometric comparisons set on foot in the beginning of 1836, and carried on whenever fitting opportunities occurred, with the express object of establishing a scale of southern magnitudes, and in which

[1] John Frederick William Herschel (1792–1871), English astronomer, son of Sir William Herschel, remeasured for the northern sky a number of his father's double stars, and repeated and extended the survey of nebulæ. To complete for the whole sky the survey of double stars and nebulæ, he made at the Cape of Good Hope a thorough examination of southern objects.

this star had been frequently compared with others both superior and inferior to it in brightness. In these comparisons its rank was always judged to be superior to that of β Crucis, γ Crucis, β

The diffuse galactic nebula η Carinae (Argus). From a Harvard-Peruvian photograph.

Argus, ϵ Canis, and Pollux, and always inferior to Spica, α Crucis, Antares, and Aldebaran; equal or a little superior to Regulus, and a good match for Fomalhaut. Estimating its magnitude numeri-

cally from these data, on a scale in which each magnitude is supposed to be divided into ten degrees or decimals, assigning to Rigel the magnitude 1.0, and to β Argus 2.0, that of η would be 1.4, in the whole interval of time from February 1834 to November 1837.

It was on the 16th December, 1837, that resuming the photometrical comparisons in question, in which, according to regular practice, the brightest stars in sight in whatever part of the heavens were first noticed, and arranged on a list, my astonishment was excited by the appearance of a new candidate for distinction among the very brightest stars of the first magnitude, in a part of the heavens with which being perfectly familiar, I was certain that no such brilliant object had before been seen. After a momentary hesitation, the natural consequence of a phenomenon so utterly unexpected, and referring to a map for its configurations with the other conspicuous stars in the neighbourhood, I became satisfied of its identity with my old acquaintance η Argus. Its light was however nearly tripled. While yet low it equalled Rigel, and when it had attained some altitude was decidedly greater. It was far superior to Achernar. Fomalhaut and α Gruis were at the time not quite so high, and α Crucis much lower, but all were fine and clear, and η Argus would not bear to be lowered to their standard. It very decidedly surpassed Procyon, which was about the same altitude, and was far superior to Aldebaran. It exceeded α Orionis, and the only star (Sirius and Canopus excepted) which could at all be compared with it was Rigel, which, as I have stated already, it somewhat surpassed.

From this time its light continued to increase. On the 28th December it was far superior to Rigel, and could only be compared with α Centauri which it equalled, having the advantage of altitude, but fell somewhat short of it as the altitudes approached equality. The maximum of brightness seems to have been obtained about the 2nd January, 1838, on which night both stars being high and the sky clear and pure, it was judged to be very nearly matched indeed with α Centauri, sometimes the one, sometimes the other being judged brighter, but on the whole α was considered to have some little superiority. After this the light began to fade. Already on the 7th, 13th January, α Centauri was unhesitatingly placed above, and Rigel as unhesitatingly below it. On the 20th, it was "visibly diminished—now much less than α Centauri, and not *much* greater than Rigel. The change is palpable." And on the 22nd, Arcturus (the nearest star in light and colour to α

Centauri which the heavens afford) when only 10° high surpassed η, the latter being on the meridian; η was still however superior to β Centauri, α Crucis and Spica, and continued so, (and even superior to Rigel) during the whole of February, nor was it until the 14th April, 1838, that it had so far faded as to bear comparison with Aldebaran, though still somewhat brighter than that star.

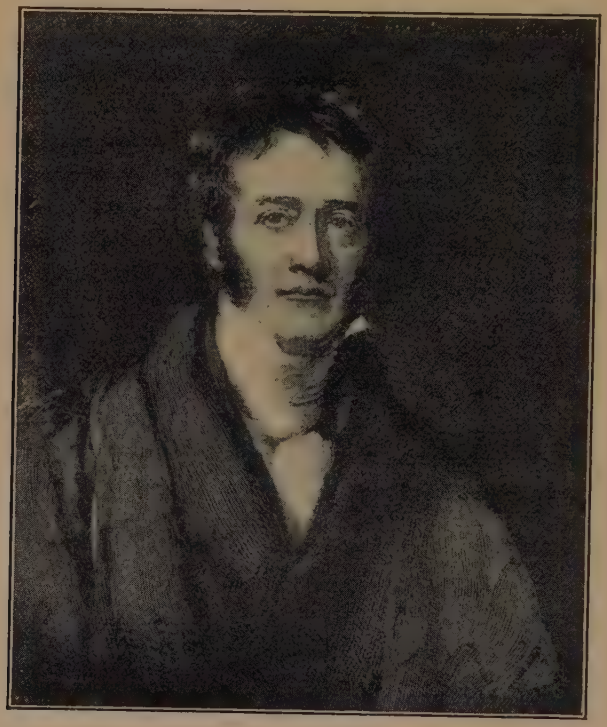
Beyond this date I am unable to speak of its further changes from personal observation. It appears, however, since that time to have made another and a still greater step in advance, and to have surpassed Canopus, and even to have approached Sirius in lustre, the former of which stars I estimate at double, the latter at more than the quadruple of α Centauri, so that Jupiter and Venus may possibly come to have a rival among the fixed stars in Argo, as they have on recorded occasions had in Cassiopeia, Serpentarius, and Aquila . . .

A strange field of speculation is opened by this phenomenon. The temporary stars heretofore recorded, have all become totally extinct. Variable stars so far as they have been carefully attended to, have exhibited periodical alternations in some degree at least regular, of splendour and comparative obscurity. But here we have a star fitfully variable to an astonishing extent, and whose fluctuations are spread over centuries, apparently in no settled period, and with no regularity of progression. What origin can we ascribe to these sudden flashes and relapses? What conclusions are we to draw as to the comfort or habitability of a system depending for its supply of light and heat on so uncertain a source? It is much to be regretted that we are without records of its changes in the intervals between the observations of Halley and Lacaille, and those of Lacaille and Burchell. Its future career will be a subject of high physical interest. To this account I will only add that in the beginning of 1838, the brightness of this star was so great as materially to interfere with the observation of that part of the nebula surrounding it which is situated in its immediate vicinity . . .

The general appearance of these objects [the two nubeculæ or Magellanic Clouds] to the naked eye in a clear night and in the absence of the moon (whose light completely effaces the lesser and almost also the larger of them), is that of pretty conspicuous nebulous patches of about the same intensity with some of the brighter portions of the milky way . . .

I ought to add that all my own attempts to delineate other than very small portions of the nubecula major from the telescope, have been completely baffled by the overwhelming complexity of its details. The lesser cloud is less complex—but for that very reason less interesting.

Of the Nubecula Minor.—The Nubecula Minor is situated between the parallels of 162° and 165° N P D, and between the



Sir John Herschel.

meridians of $0^h\ 28^m$ and $1^h\ 15^m$ R A. It is of a generally round form to the unaided eye, nor can any very material deviation of its centre of brightness from its centre of figure be noticed. As seen in the telescope, however, the most conspicuous and resolvable region appears to lie somewhere about $0^h\ 45^m\ 30^s$ R A, and 164° 16′ N P D, which is somewhat to the south of its middle. It is preceded at a few minutes distance in R A, by the magnificent globular cluster 47 Toucani (Bode), but is completely cut off from all connection with it; and with this exception, its situation is

in one of the most barren regions of the heavens. I cannot better describe its general insulation than in the words of a memorandum in the Gage-book sweep 745. "The access to the Nubecula Minor on all sides is through a desert." Neither with the naked eye nor with a telescope is any connexion to be traced either with the greater nubecula or with the milky way . . .

Of the Nubecula Major.—The Nubecula Major is situated between the parallels of 156° and 162° N P D, and the meridians of $4^h\ 40^m$ and $6^h\ 0^m$ in R A. Its brightest, and for the most part, unresolved portion (which has been pitched on several times in sweeping as "the main body of the cloud") is situated in $5^h\ 20^m$ R A and 159°40′ N P D, but the whole region between $5^h\ 10^m$ and $5^h\ 30^m$ in R A, and from 158°50′ to 160°10′ in N P D, is almost equally entitled to be so called. The brightest and richest region of resolved and clustering stars is situated about $5^h\ 30^m$ R A, 157° 0′ N P D.

The Nubecula Major, like the Minor, consists partly of large tracts and ill-defined patches of irresolvable nebula, and of nebulosity in every stage of resolution, up to perfectly resolved stars like the Milky Way, as also of regular and irregular nebulæ properly so called, of globular clusters in every stage of resolvability, and of clustering groups sufficiently insulated and condensed to come under the designation of "clusters of stars," in the sense in which that expression is always to be understood in reading my Father's and my own catalogues. In the number and variety of these objects, and in general complexity of structure, it far surpasses the Lesser Nubecula; some idea of which may be formed by comparing the numbers of registered nebulæ and clusters in each. For while, within the limits assigned above to the latter, the number of such nebulæ and clusters amounts only to 37, and taking in six outliers, which may be regarded as forming part of its system, at most to 43 (a very remarkable concentration of such objects already, within an area not much exceeding ten square degrees), the former within an area of about 42 square degrees, allows us to enumerate no fewer than 278 (without reckoning between 50 and 60 outliers, immediately adjacent, and which may very fairly be regarded as appendages of the nebulous system of the Nubecula Major) making an average of about 6½ to the square degree, which very far exceeds anything that is to be met with in any other region of the heavens. Even the most crowded parts of the stratum of Virgo, in the wing of that constellation, or in Coma

Berenices, offer nothing approaching to it. It is evident from this, and from the intermixture of stars and unresolved nebulosity which probably might be resolved with a higher optical power, that the nubeculæ are to be regarded as systems *sui generis*, and which have no analogues in our hemisphere . . .

The immediate neighbourhood of the Nubecula Major, is somewhat less barren of stars than that of the Minor, but it is by no means rich, nor does any branch of the Milky Way whatever form any certain and conspicuous junction with, or include it; though on very clear nights I have sometimes fancied a feeble extension of the nearer portion of the Milky Way in Argo (where it is not above 15° or 20° distinct) in the direction of the nubecula. On the whole, however, I do not consider this appearance as more than would be accounted for by the general increase of the number of small stars which, in almost every part of the course of the Milky Way, accompany its borders, and, in a telescope, announce its approach. I have encountered nothing that I could set down as *diffused nebulosity* anywhere in the neighbourhood of either nubecula.

ADAMS[1]

The History of the Discovery of Neptune

(From *Memoirs* of the Royal Astronomical Society, Vol. 16, 1847.)

The irregularities in the motions of *Uranus* have for a long time engaged the attention of Astronomers. When the path of the planet became approximately known, it was found that, previously to its discovery by *Sir W. Herschel* in 1781, it had several times been observed as a fixed star by *Flamsteed*, *Bradley*, *Mayer*, and *Lemonnier*. Although these observations are doubtless very far inferior in accuracy to the modern ones, they must be considered valuable, in consequence of the great extension which they give to the observed arc of the planet's orbit. *Bouvard*, however, to whom we owe the tables of *Uranus* at present in use, found that it was impossible to satisfy these observations without attributing much larger errors to the modern observations than they admit of, and consequently founded his tables exclusively on the latter. But, in a very few years, sensible errors began again to show themselves, and, though the tables were formed so recently as 1821, their error at the present time exceeds two minutes of space, and is still rapidly increasing. There appeared, therefore, no longer any sufficient reason for rejecting the ancient observations, especially since, with the exception of *Flamsteed's* first observation, which is more than twenty years anterior to any of the others, they are mutually confirmatory of each other.

Now that the discovery of another planet has confirmed in the most brilliant manner the conclusions of analysis, and enabled us with certainty to refer these irregularities to their true cause, it is unnecessary for me to enter at length upon the reasons which led me to reject the various other hypotheses which had been formed to account for them. It is sufficient to say, that they all appeared to be very improbable in themselves, and incapable of being tested by any exact calculation. Some had even supposed that, at the

[1] John Couch Adams (1819–1892), English mathematician, developed new methods of dealing with lunar theory. He made the first serious attempt to deduce from the irregularities in the motion of Uranus the position of Neptune.

great distance of *Uranus* from the sun, the law of attraction becomes different from that of the inverse square of the distance. But the law of gravitation was too firmly established for this to be admitted till every other hypothesis had failed, and I felt convinced that in this, as in every previous instance of the kind, the discrepancies which had for a time thrown doubts on the truth of the law, would eventually afford the most striking confirmation of it.

My attention was first directed to this subject several years since, by reading *Mr. Airy's* valuable Report on the recent progress of Astronomy. I find among my papers the following memorandum, dated July 3, 1841: "Formed a design, in the beginning of this week, of investigating, as soon as possible after taking my degree, the irregularities in the motion of *Uranus*, which are yet unaccounted for, in order to find whether they may be attributed to the action of an undiscovered planet beyond it, and, if possible, thence to determine approximately the elements of its orbit, etc., which would probably lead to its discovery." Accordingly, in 1843, I attempted a first solution of the problem, assuming the orbit to be a circle, with a radius equal to twice the mean distance of *Uranus* from the sun. Some assumption as to the mean distance was clearly necessary in the first instance, and *Bode's* law appeared to render it probable that the above would not be far from the truth. This investigation was founded exclusively on the modern observations, and the errors of the tables were taken from those given in the equations of condition of *Bouvard's* tables as far as the year 1821, and subsequently from the observations given in the *Astronomische Nachrichten*, and from the Cambridge and Greenwich Observations. The result shewed that a good general agreement between theory and observation might be obtained; but the larger differences occurring in years where the observations used were deficient in number, and the Greenwich Planetary Observations being then in process of reduction, I applied to *Mr. Airy*, through the kind intervention of *Professor Challis*, for the observations of some years in which the agreement appeared least satisfactory. The Astronomer Royal, in the kindest possible manner, sent me in February 1844 the results of all the Greenwich Observations of *Uranus*.

Meanwhile the Royal Academy of Sciences of Göttingen had proposed the theory of *Uranus* as the subject of their mathematical prize, and although the little time which I could spare from important duties in my college prevented me from attempting the

complete examination of the theory which a competition for the prize would have required, yet this fact, together with the possession of such a valuable series of observations, induced me to undertake a new solution of the problem. I now took into account the most important terms depending on the first power of the eccentricity of the disturbing planet, retaining the same assumption as before with respect to the mean distance. For the modern observations, the errors of the tables were taken exclusively from the Greenwich Observations as far as the year 1830, with the exception of an observation by *Bessel* in 1823; and subsequently from the Cambridge and Greenwich Observations, and those given in various numbers of the *Astronomische Nachrichten*. The errors of the tables for the ancient observations were taken from those given in the equations of condition of *Bouvard's* tables. After obtaining several solutions differing little from each other, by gradually taking into account more and more terms of the series expressing the perturbations, I communicated to *Professor Challis*, in September 1845, the final values which I had obtained for the mass, heliocentric longitude, and elements of the orbit of the assumed planet. The same results, slightly corrected, I communicated in the following month to the Astronomer Royal. The eccentricity coming out much larger than was probable, and later observations shewing that the theory founded on the first hypothesis as to the mean distance was still sensibly in error, I afterwards repeated my investigation, supposing the mean distance to be about $\frac{1}{30}$th part less than before. The result, which I communicated to *Mr. Airy* in the beginning of September of the present year, appeared more satisfactory than my former one, the eccentricity being smaller, and the errors of theory, compared with late observations, being less, and led me to infer that the distance should be still further diminished.

In November 1845, *M. Le Verrier* presented to the Royal Academy of Sciences, at Paris, a very complete and elaborate investigation of the theory of *Uranus*, as disturbed by the action of *Jupiter* and *Saturn*, in which he pointed out several small inequalities which had previously been neglected; and in June, of the present year, he followed up this investigation by a memoir, in which he attributed the residual disturbances to the action of another planet at a distance from the sun equal to twice that of *Uranus*, and found a longitude for the new planet agreeing very nearly with the result which I had obtained on the same hypothesis.

On the 31st of August, he presented to the Academy a more complete investigation, in which he determined the mass and the elements of the orbit of the new planet, and also obtained limiting values of the mean distance and heliocentric longitude. I mention these dates merely to shew that my results were arrived at independently, and previously to the publication, of those of *M. Le Verrier*, and not with the intention of interfering with his just claims to the honours of the discovery; for there is no doubt that his researches were first published to the world, and led to the actual discovery of the planet by *Dr. Galle*, so that the facts stated above cannot detract, in the slightest degree, from the credit due to *M. Le Verrier*.

LEVERRIER[1]

Prediction of the Position of Neptune

(Translated from a letter, dated September 8, 1846, from Leverrier to the editor of the *Astronomische Nachrichten*, published in Vol. 25, No. 580, Oct. 12, 1846.)

In the last letter that I had the pleasure of writing you, I reported that I had undertaken extensive researches on the motions of Uranus, and that I was coming to the conclusion that a perturbing planet existed, for which I indicated the position. I have been very busy since then in perfecting my work, and I formed the desire to carry it through before the time of opposition of the new body in order that astronomical observers could explore with ease the region of the sky called to their attention. But I had not counted on an indisposition which has much retarded me, with the result that the opposition of the planet has already passed some days ago. Happily the disadvantage, which results from the diminution of the angular distance to the Sun, will be compensated for by the very rapid decrease in the length of the day. We will be for a long time yet in a favorable situation for the physical researches which should be attempted.

I take the liberty of addressing to you an extract of my work, with the request that it be inserted in your learned journal. I hope to be able before long to publish my researches in detail—may they inspire sufficient confidence in astronomical observers to encourage them to make a careful study of the part of the sky where it will be possible without doubt to discover a planet of which the mass is very considerable.

You will see, sir, that I have supposed that the disturbing body is situated in the ecliptic. I have not yet had the leisure to examine if it will be possible to deduce from the observations any precise data concerning the latitude. But we can be sure, even at present,

[1] Urbain Jean Joseph Leverrier (1811–1877), French mathematician, made important contributions to planetary theory. His calculations and predictions led to the discovery of Neptune by Dr. J. G. Galle (1812–1910) at the Berlin Observatory on September 23, 1846.

that this latitude will be fairly low since the latitudes of Uranus accord very approximately with the tables in use. This is, moreover, the only point which still remains for me to consider, and I shall proceed to occupy myself with it . . .

Researches on the Motions of Uranus.[1] — I undertake, in the publication of which I present here an abstract, to investigate the nature of the irregularities in the motion of Uranus; to determine their cause, while trying to discover, from the course which they take, the direction and the magnitude of the forces which produce them.

The theory of Uranus at the present time absorbs the attention of astronomers. It has been the subject of many hypotheses, more or less plausible, which, however, aside from geometric considerations, cannot have any real value. Several societies have even proposed the theory as a subject for competition. I believe, therefore, that because of the importance of the question I should rapidly recount its history. One can then better judge the goal of my work, the course which I have travelled, and the results at which I have arrived.

In 1820, there were available regular meridian observations extending over a period of forty years. The planet had, moreover, been observed nineteen times between 1690 and 1771 by Flamsteed, Bradley, Mayer, and Lemonnier. These astronomers had seen it as a star of the sixth magnitude. On the other hand, the analytical expressions for the perturbations which Jupiter and Saturn produce on Uranus are to be found developed in the first volume of the Mécanique Céleste. Using all these data, one should have expected to be able to construct exact tables for the planet. This is what Bouvard, Member of the Academy of Sciences, undertook. But he encountered unforseen difficulties.

When the tables of a planet are based on too few observations it may happen that these tables, in the course of time, no longer give correctly the position of the planet. But at least the observations used are represented by the tables with all the rigour which they demand; it can even be said that the fewer the observations, the more easily they can be represented.

This was not the case, however, in the construction of the tables of Uranus. It was found impossible to represent at the same time the nineteen older observations and the numerous modern ones. In this embarrassing situation the learned member of the academy

[1] Translated from *Astronomische Nachrichten*, No. 580, Oct. 12, 1846.

throws doubt upon the accuracy of the older observations; he discards them completely and takes into account only the modern observations. But one should note that though the observations of Flamsteed, Bradley, Mayer, and Lemonnier are not as exact as those of the astronomers of our epoch, one may not with any plausibility be allowed to consider them infested with such enor-

Leverrier. From Lebon's *Histoire de l'Astronomie.* (*By permission of Gauthier-Villars, Imprimeur-Libraire.*)

mous errors as those of which the present tables accuse them. The author of these tables actually suggests, however, that this is his opinion, although he adds, in reviewing the difficulties which he had encountered:

"Such is then the alternative which the formation of the tables of the planet Uranus presents, that if one combines the older observations with the modern ones the former will be passably

represented while the latter will not be represented with the precision they demand; and if one rejects the older observations so as to use only the modern ones, the result will be tables which will have all the desirable accuracy relative to the modern observations, but which will not be able to satisfy sufficiently the older observations. It is necessary to choose between the alternatives; I have thought best to hold to the second, as being the one which has the greater probability of truth; and the future shall have the burden of demonstrating whether the difficulty of reconciling the two systems is really connected with the inaccuracy of observations, or whether it depends on some strange and unperceived force which may be exerted on the planet."

The twenty-five years which have elapsed since that epoch have shown us that the present tables, which do not represent the older positions, are in no better agreement with the positions observed in 1845. May this disagreement be attributed to lack of precision in the theory? Or rather has not the theory been applied to the observations with sufficient exactitude in the work which has served as a basis for the present tables? Or finally, might it be that Uranus is subjected to other influences besides those which result from the action of the Sun, of Jupiter, and of Saturn? And, in this case, might one succeed, by a careful study of the disturbed motion of the planet, in determining the cause of these unforseen irregularities? And could one come to the point of fixing the spot in the sky where the investigations of observing astronomers ought to discover the strange body, the source of all the difficulties?

In the course of the summer of the year 1845, M. Arago persuaded me that the importance of this question made it the duty of every astronomer to cooperate, as much as possible, in clearing up some point of the difficulty. I abandoned, then, immediately the researches on comets which I had undertaken, of which several fragments have already been published, so as to occupy myself with Uranus. Such is the origin of the present research.

Galle's Discovery of Neptune.[1] — No mail went to Hamburg yesterday and, therefore, I could not announce to you the dis-

[1] Translated from a letter dated Sept. 26, 1846, written by Prof. J. S. Encke, Director of the Royal Observatory, to H. C. Schumacher, of Hamburg, editor of the *Astronomische Nachrichten*, printed in *Astronomische Nachrichten*, No. 580, Oct. 12, 1846.

covery of the Leverrier planet. Accordingly, I can today give you more information. In the *Comptes Rendus* for August 31, 1846, M. Leverrier has given the following elements, deduced from the deviations of Uranus from its orbit, computed on the basis of the known masses:

Semi-major axis	36.154
Period of revolution	217.387 years (sidereal)
Eccentricity	0.10761
Perihelion	284°45′
Mean longitude on January 1, 1847	318°47′
Mass	1/9300

and from this it follows

Heliocentric True Longitude, January 1, 1847	326°32′
Distance from the Sun	33.06

In a letter which arrived on September 23, M. Leverrier especially urged Dr. Galle to search for the planet. Probably he was guided by the supposition mentioned in his article that the planet could be identified through showing a disk. The same evening Galle compared with the sky the excellent maps which Dr. Bremiker has plotted (Hour XXI of the Academy Star Charts), and almost immediately noticed, very near to the position which Leverrier predicts, a star of the eighth magnitude which was missing on the chart. It was immediately measured three different times by Galle with reference to a star in Bessel's catalogue (each measure consisting of five observations), and was once measured by me. The results of these comparisons are as follows:

Sidereal Time	$22^h\ 52^m$	R. A. diff. $+ 1^m\ 25^s.84$	Dec. diff. $+ 1'\ 35''.9$	Galle
	23 47	25.30	37.9	Galle
	0 52	25.34	35.9	Galle
	1 8	25.26	37.3	Encke

Although on the whole there is shown here a progression, nevertheless, the discrepancies in this first series were so noticeable that it cannot be depended upon. Therefore, we waited until the next evening. At that time, to be sure, the weather interfered, cloudiness interrupting the observations. Nevertheless, motion exactly in the direction of the Leverrier elements was decisive, for we found, using the same star,

Sept. 24	$20^h\ 7^m$	$+ 1^m\ 21^s.56$	$+1'16''.4$	Galle	5 Observations
	21 11	21.30	14.8	Galle	5
	22 20	21.08	14.4	Encke	4

Similarly, on the 25th of September, when Galle compared the star five times and I, ten times, the motion was confirmed . . .

The star seemed to be only a trifle fainter than Piazzi XXI, 344, and, therefore, fully as bright as the eighth magnitude. Yesterday the atmospheric conditions were favorable. We recognized a disk, the diameter of which, using bright cross wires and a magnification of 320, I found to be 2″.9; Galle found 2″.7. When we subsequently used a bright field, I measured the planet greater than 3″.2, and Galle considerably smaller than 2″.2; but by this time the air had become much more unfavorable so that the first measurements are more to be trusted. I believe that the diameter is probably 2″.5, or perhaps somewhat greater, but not as large as 3″.0. In this respect also the prediction of Leverrier, who assumed 3″.3, is fully confirmed.[1]

It would be superfluous to add anything more. This is the most brilliant of all planetary discoveries, because purely theoretical researches have enabled Leverrier to predict the existence and the position of a new planet. Permit me merely to add that the prompt discovery was possible only because of the excellent Academy Star Charts by Bremiker; the disk can be recognized only when one knows that it exists.

[1 The value now adopted is 2″.3.]

ROSSE[1]

On the Spiral Forms of Certain Nebulæ

(From *Philosophical Transactions*, 1850.)

In laying before the Royal Society an account of the progress which has been made up to the present date in the reexamination of *Sir John Herschel's* "Catalogue of Nebulæ" published in the *Philosophical Transactions* for 1833, it will be necessary to say something of the qualities of the instrument employed.

The telescope has a clear aperture of 6 feet, and a focal length of 53 feet. It has hitherto been used as a Newtonian, but in constructing the galleries provision was made for the easy application of a little additional apparatus to change the height of the observer, so that the focal length of the speculum remaining the same, the instrument could be conveniently worked as a Herschelian.

Although with an aperture so great in proportion to the focal length, the performance of a parabolic speculum placed obliquely would no doubt be very unsatisfactory, still additional light is so important in bringing out faint details, that it is not improbable in the further examination of the objects of most promise with the full light of the speculum, *undiminished by a second reflexion*, some additional features of interest will come out . . .

The sketches which accompany this paper are on a very small scale, but they are sufficient to convey a pretty accurate idea of the peculiarities of structure which have gradually become known to us: in many of the nebulæ they are very remarkable, and seem even to indicate the presence of dynamical laws we may perhaps fancy to be almost within our grasp. To have made full-sized copies of the original sketches would have been useless, as many micrometrical measures are still wanting, and there are many matters of detail to be worked in before they will be entitled to rank as

[1] William Parsons, Earl of Rosse (1800–1867), Irish amateur scientist, constructed for his reflecting telescope a mirror 6 feet in diameter and 53 feet in focal length with which he studied the structural detail and discovered the spiral form of many nebulæ.

astronomical records, to be referred to as evidence of change, should there, hereafter, be any reason to suspect it . . .

It will be at once remarked, that the spiral arrangement so strongly developed in 51 *Messier*, is traceable, more or less distinctly, in several of the sketches. More frequently indeed there is a nearer approach to a kind of irregular interrupted annular disposition of the luminous material than to the regularity so striking in 51 *Messier;* but it can scarcely be doubted that these nebulæ are systems of a very similar nature, seen more or less perfectly, and variously placed to the line of sight. In general, the details which characterize objects of this class are extremely faint, scarcely perhaps to be seen with certainty on a moderately good night with less than the full aperture of 6 feet: in 51 *Messier*, however, and perhaps a few more, it is not so. A 6-feet aperture so strikingly brings out the characteristic features of 51 *Messier*, that I think considerably less power would suffice, on a very fine night, to bring out the principal convolutions. This nebula has been seen by a great many visitors, and its general resemblance to the sketch at once recognized even by unpractised eyes. *Messier* describes this object as a double nebula without stars; *Sir William Herschel* as a bright round nebula, surrounded by a halo or glory at a distance from it, and accompanied by a companion; and *Sir John Herschel* observed the partial subdivision of the *s.f.* limb of the ring into two branches. Taking *Sir J. Herschel's* figure, and placing it as it would be if seen with a Newtonian telescope, we shall at once recognise the bright convolutions of the spiral, which were seen by him as a divided ring. We thus observe, that with each successive increase of optical power, the structure has become more complicated and more unlike anything which we could picture to ourselves as the result of any form of dynamical law, of which we find a counterpart in our system. The connection of the companion with the greater nebula, of which there is not the least doubt and in the way represented in the sketch, adds, as it appears to me, if possible, to the difficulty of forming any conceivable hypothesis. That such a system should exist, without internal movement, seems to be in the highest degree improbable: we may possibly aid our conceptions by coupling with the idea of motion that of a resisting medium; but we cannot regard such a system in any way as a case of mere statical equilibrium. Measurements, therefore, are of the highest interest, but unfortunately they are attended with great difficulties. Measurements of the points

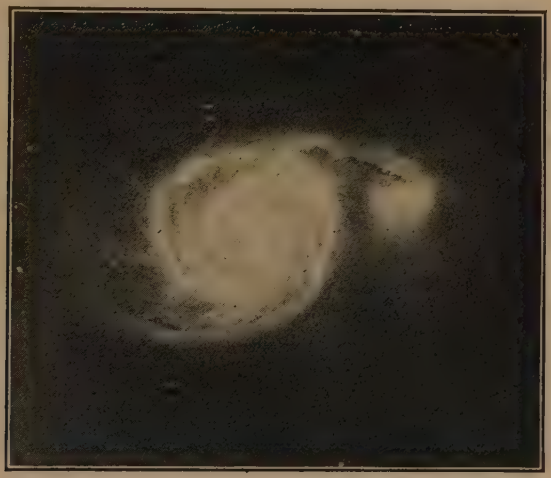
Drawings of Messier 51 (above) and Messier 99.

of maximum brightness in the motling of the different convolutions must necessarily be very loose; for although on the finest nights we see them breaking up into stars, the exceedingly minute stars cannot be seen steadily, and to identify one in each case would be impossible with our present means. The nebula itself, however, is pretty well studded with stars, which can be distinctly seen of various sizes, and of a few of these, with reference to the principal nucleus, measurements were taken by my assistant, *Mr. Johnstone Stoney*, in the spring of 1849, during my absence in London; for some time before the weather had been continually cloudy. These measurements have been again repeated by him this year, 1850, during the months of April and May. Just as was the case last year, in February and March the sky was almost constantly overcast. He has also taken some measures from the centre of the principal nucleus to the apparent boundary of the coils, in different angles of position. The micrometer employed was furnished with broad lines formed of a coil of silver wire in the way I have described, seen without illumination. Some of the stars in the nebula are so bright, I have little doubt they would bear illumination; if so, their positions with respect to some one star might be obtained with great accuracy of course by employing spiders' lines; this season, however, it is too late to make the attempt. Several of these stars are no doubt within the reach of the great instruments at Pulkova and at Cambridge, U. S., and I hope the distinguished astronomers who have charge of them will consider the subject worthy of their attention. Their better climate gives them many advantages, of which not the least is the opportunity of devoting time to measurements without any serious interruption to other work. I need, perhaps, hardly add, that the measurements taken from the estimated centre of a nucleus, and still more from the estimated termination of nebulosity, are but the roughest approximations; they are, however, the only measurements nebulosity admits of, and if sufficiently numerous, I think they will bring to light any considerable change of place, or form, which may occur.

The spiral arrangement of 51 *Messier* was detected in the spring of 1845. In the following spring an arrangement, also spiral but of a different character, was detected in 99 *Messier*. This object is also easily seen, and probably a smaller instrument, under favourable circumstances, would show everything in the sketch. Numbers 3239 and 2370 of *Herschel's* "Southern Catalogue" are very

probably objects of a similar character, and as the same instrument does not seem to have revealed any trace of the form of 99 *Messier*, they are no doubt much more conspicuous. It is not therefore unreasonable to hope, that whenever the southern hemisphere shall be re-examined with instruments of great power, these two remarkable nebulæ will yield some interesting result.

The other spiral nebulæ discovered up to the present time are comparatively difficult to be seen, and the full power of the instrument is required, at least in our climate, to bring out the details.



A photograph of the spiral nebula Messier 51. *Mount Wilson Observatory.*

It should be observed that we are in the habit of calling all objects spirals in which we have detected a curvilinear arrangement not consisting of regular re-entering curves; it is convenient to class them under a common name, though we have not the means of proving that they are similar systems. They, at present, amount to fourteen, four of which have been discovered this spring; there are besides other nebulæ in which indications of the same character have been observed, but they are still marked doubtful in our working list, having been seen when the air was not very transparent; 51 *Messier* is the most conspicuous object of that class . . .

In passing from the spiral to the regular annular nebulæ, we perceive we are at once engaged with objects of a very different character: still here even there seems to be something like a con-

necting link;[1] the great round planetary nebula, H838, with a double perforation appears to partake of the structure both of the annular and spiral nebulæ. There were but two annular nebulæ known in the northern hemisphere when *Sir John Herschel's* "Catalogue" was published; now there are seven, as we have found that five of the planetary nebulæ are really annular. Of these objects, the annular nebula in Lyra is the one in which the form is by far the most easily recognized. I have not yet sketched it with the 6-feet instrument, because I have never seen it under favourable circumstances: the opportunities of observing it well on the meridian are comparatively rare owing to twilight. It was, however, observed seven times in 1848 and once in 1849. The only additional particulars I collect from the observations are that the central opening has considerably more nebulosity in it than it appeared to have with the 3-feet instrument, and that there is one pretty bright star in it, *s.f.* the centre, and a few other very minute stars. In the sky round the nebula and near it there are several very small stars which were not before seen, and, therefore, the stars in the dark opening may possibly be merely accidental. In the annulus, especially at the extremities of the minor axis, there are several minute stars, but there was still much nebulosity not seen as distinct stars.

[1 One of the earliest discussions (after the Herschels) of the classification of nebulæ was published in the *Astronomical Journal* in 1852 by Stephen Alexander.]

KELVIN[1]

The Age of the Earth as an Abode Fitted for Life

(From the *Report* of the Smithsonian Institution for 1897.)

The age of the earth as an abode fitted for life is certainly a subject which largely interests mankind in general. For geology it is of vital and fundamental importance—as important as the date of the battle of Hastings is for English history—yet it was very little thought of by geologists of thirty or forty years ago; how little is illustrated by a statement, which I will now read, given originally from the presidential chair of the Geological Society by Professor Huxley in 1869, when for a second time, after a seven years' interval, he was president of the society. "I do not suppose that at the present day any geologist would be found . . . to deny that the rapidity of the rotation of the earth *may* be diminishing, that the sun *may* be waxing dim, or that the earth itself *may* be cooling. Most of us, I suspect, are Gallios, 'who care for none of these things,' being of opinion that, true or fictitious, they have made no practical difference to the earth, during the period of which a record is preserved in stratified deposits."

I believe the explanation of how it was possible for Professor Huxley to say that he and other geologists did not care for things on which the age of life on the earth essentially depends is because he did not know that there was valid foundation for any estimates worth considering as to absolute magnitudes. If science did not allow us to give any estimate whatever as to whether 10 million or 10 billion years is the age of this earth as an abode fitted for life, then I think Professor Huxley would have been perfectly right in saying that geologists should not trouble themselves about it, and biologists should go on in their own way, not inquiring into things utterly beyond the power of human understanding

[1] William Thomson, Lord Kelvin (1824–1907), British physicist, held the chair of natural philosophy in the University of Glasgow for 53 years. His scientific contributions were of high importance in many subjects, ranging from transatlantic cables to the age of the Sun and the Earth.

and scientific investigation. This would have left geology much in the same position as that in which English history would be if it were impossible to ascertain whether the battle of Hastings took place 800 years ago, or 800 thousand years ago, or 800 million years ago. If it were absolutely impossible to find out which of these periods is more probable than the other, then I agree we might be Gallios as to the date of the Norman Conquest. But a change took place just about the time to which I refer, and from then till now geologists have not considered the question of absolute dates in their science as outside the scope of their investigations . . .

The rate at which heat is at the present time lost from the earth by conduction outward through the upper crust, as proved by observation of underground temperature in different parts of the world and by measurement of the thermal conductivity of surface rocks and strata, sufficed to refute utterly the doctrine of uniformity as taught by Hutton, Lyell, and their followers.

In an earlier communication to the Royal Society of Edinburgh I had considered the cooling of the earth due to this loss of heat, and by tracing backward the process of cooling had formed a definite estimate of the greatest and least number of million years which can possibly have passed since the surface of the earth was everywhere red-hot. I expressed my conclusion in the following statement: "We are very ignorant as to the effects of high temperatures in altering the conductivities and specific heats and melting temperatures of rocks, and as to their latent heat of fusion. We must, therefore, allow very wide limits in such an estimate, as I have attempted to make; but I think we may with much probability say that the consolidation can not have taken place less than 20 million years ago, or we should now have more underground heat than we actually have; nor more than 400 million years ago, or we should now have less underground heat than we actually have. That is to say, I conclude that Leibnitz's epoch of emergence of the *consistentior status* (the consolidation of the earth from red-hot or white-hot molten matter) was probably between those dates."

During the thirty-five years which have passed since I gave this wide-ranged estimate, experimental investigation has supplied much of the knowledge then wanting regarding the thermal properties of rocks to form a closer estimate of the time which has passed since the consolidation of the earth, and we have now good

reason for judging that it was more than 20 and less than 40 million years ago,[1] and probably much nearer 20 than 40 . . .

When the surface of the earth was still white-hot liquid all round, at a temperature fallen to about 1200° C., there must have been hot gases and vapor of water above it in all parts, and possibly vapors of some of the more volatile of the present known terrestrial solids and liquids, such as zinc, mercury, sulphur, phosphorus. The very rapid cooling which followed instantly on the solidification at the surface must have caused a rapid downpour of all the vapors other than water, if any there were; and a little later, rain of water out of the air, as the temperature of the surface cooled from red heat to such moderate temperatures as 40° and 20° and 10° C. above the average due to sun heat and radiation into the ether around the earth. What that primitive atmosphere was, and how much rain of water fell on the earth in the course of the first century after consolidation, we can not tell for certain; but natural history and natural philosophy give us some foundation for endeavors to discover much toward answering the great questions: Whence came our present atmosphere of nitrogen, oxygen, and carbonic acid? Whence came our present oceans and lakes of salt and fresh water? How near an approximation to present conditions was realized in the first hundred centuries after consolidation of the surface?

We may consider it as quite certain that nitrogen gas, carbonic acid gas, and steam escaped abundantly in bubbles from the molten liquor of granite before the primitive consolidation of the surface, and from the mother liquid squeezed up from below in subsequent eruptions of basaltic rock; because all, or nearly all, specimens of granite and basaltic rock which have been tested by chemists in respect to this question have been found to contain, condensed in minute cavities within them, large quantities of nitrogen, carbonic acid, and water. It seems that in no specimen of granite or basalt tested has chemically free oxygen been discovered, while in many chemically free hydrogen has been found, and either native iron or magnetic oxide of iron in those which do not contain hydrogen. From this it might seem probable that there was no free oxygen in the primitive atmosphere, and that if there was free

[1 Since Kelvin's results were obtained, the radioactivity in the surface rocks has been discovered, studied, and found to affect profoundly all deductions concerning the age of the earth; in fact, the radioactive minerals now afford the best means of dating the earth's developmental stages and show that Kelvin's values must be multiplied by eight or ten.]

hydrogen it was due to the decomposition of steam by iron or magnetic oxide of iron. Going back to still earlier conditions we might judge that, probably, among the dissolved gases of the hot nebula which became the earth the oxygen all fell into combination with hydrogen and other metallic vapors in the cooling of the nebula, and that although it is known to be the most abundant material of all the chemical elements constituting the earth none of it was left out of combination with other elements to give free oxygen in our primitive atmosphere.

It is, however, possible, although it might seem not probable, that there was free oxygen in the primitive atmosphere. With or without free oxygen, however, *but with sunlight*, we may regard the earth as fitted for vegetable life as now known in some species, wherever water moistened the newly solidified rocky crust cooled down below the temperature of 80° or 70° of our present Centigrade thermometric scale, a year or two after solidification of the primitive lava had come up to the surface. The thick, tough velvety coating of living vegetable matter covering the rocky slopes under hot water flowing direct out of the earth at Banff (Canada) lives without help from any ingredients of the atmosphere above it, and takes from the water and from carbonic acid or carbonates, dissolved in it, the hydrogen and carbon needed for its own growth by the dynamical power of sunlight; thus leaving free oxygen in the water to pass ultimately into the air. Similar vegetation is found abundantly on the terraces of the Mammoth Hot Springs and on the beds of the hot water streams flowing from the geysers in the Yellowstone National Park of the United States. This vegetation, consisting of confervæ, all grows under flowing water at various temperatures, some said to be as high as 74° C. We can not doubt but that some such confervæ, if sown or planted in a rivulet or pool of warm water in the early years of the first century of the solid earth's history and if favored with sunlight would have lived, and grown, and multiplied, and would have made a beginning of oxygen in the air if there had been none of it befor' their contributions. Before the end of the century if sun heat, and sunlight, and rainfall were suitable the whole earth not under water must have been fitted for all kinds of land plants which do not require much or any oxygen in the air, and which can find or make place and soil for their roots on the rocks on which they grow, and the lakes or oceans formed by that time must have been quite fitted for the life of many or all of the species of water plants living on the earth at the

present time. The moderate warming, both of land and water, by underground heat, toward the end of the century, would probably be favorable rather than adverse to vegetation, and there can be no doubt but that if abundance of seeds of all species of the present day had been scattered over the earth at that time an important proportion of them would have lived and multiplied by natural selection of the places where they could best thrive.

But if there was no free oxygen in the primitive atmosphere or primitive water several thousands, possibly hundreds of thousands, of years must pass before oxygen enough for supporting animal life, as we now know it, was produced. Even if the average activity of vegetable growth on land and in water over the whole earth was, in those early times, as great in respect to evolution of oxygen as that of a Hessian forest, as estimated by Liebig fifty years ago, or of a cultivated English hayfield of the present day, a very improbable supposition, and if there were no decay (eremacausis, or gradual recombination with oxygen) of the plants or of portions such as leaves falling from plants, the rate of evolution of oxygen, reckoned as three times the weight of the wood or dry hay produced, would be only about 6 tons per English acre per annum, or 1½ tons per square meter per thousand years. At this rate it would take only 1533 years, and, therefore, in reality a much longer time would almost certainly be required, to produce the 2.3 tons of oxygen which we have at present resting on every square meter of the earth's surface, land and sea. But, probably, quite a moderate number of hundred thousand years may have sufficed. It is interesting, at all events, to remark that at any time the total amount of combustible material on the earth, in the form of living plants or their remains left dead, must have been just so much that to burn it all would take either the whole oxygen of the atmosphere or the excess of oxygen in the atmosphere at the time, above that, if any, which there was in the beginning. This we can safely say, because we almost certainly neglect nothing considerable in comparison with what we assert when we say that the free oxygen of the earth's atmosphere is augmented only by vegetation liberating it from carbonic acid and water, in virtue of the power of sunlight, and is diminished only by virtual burning[1] of the vegetable

[1] This "virtual burning" includes eremacausis of decay of vegetable matter, if there is any eremacausis of decay without the intervention of microbes or other animals. It also includes the combination of a portion of the food with inhaled oxygen in the regular animal economy for provision for heat and power.

matter thus produced. But it seems improbable that the average of the whole earth—dry land and sea bottom—contains at present coal, or wood, or oil, or fuel of any kind originating in vegetation, to so great an amount as 0.767 of a ton per square meter of surface, which is the amount at the rate of 1 ton of fuel to 3 tons of oxygen per square meter of surface which our present atmosphere contains. Hence it seems probable that the earth's primitive atmosphere must have contained free oxygen.

Whatever may have been the true history of our atmosphere, it seems certain that if sunlight was ready, the earth was ready, both for vegetable and animal life, if not within a century, at all events within a few hundred centuries after the rocky consolidation of its surface. But was the sun ready? The well-founded dynamical theory of the sun's heat, carefully worked out and discussed by Helmholtz, Newcomb, and myself, says NO if the consolidation of the earth took place as long ago as fifty million years; the solid earth must in that case have waited twenty or thirty million years for the sun to be anything nearly as warm as he is at present. If the consolidation of the earth was finished twenty or twenty-five million years ago the sun was probably ready—though probably not then quite so warm as at present, yet warm enough to support some kind of vegetable and animal life on the earth.

My task has been rigorously confined to what, humanly speaking, we may call the fortuitous concourse of atoms in the preparation of the earth as an abode fitted for life, except in so far as I have referred to vegetation, as possibly having been concerned in the preparation of an atmosphere suitable for animal life as we now have it. Mathematics and dynamics fail us when we contemplate the earth, fitted for life but lifeless, and try to imagine the commencement of life upon it. This certainly did not take place by any action of chemistry, or electricity, or crystalline grouping of molecules under the influence of force, or by any possible kind of fortuitous concourse of atoms. We must pause, face to face with the mystery and miracle of the creation of living creatures.

GEORGE P. BOND[1]

The Future of Stellar Photography

(From a letter written in 1857 to Wm. Mitchell; *Publications* of the Astronomical Society of the Pacific, Vol. 2, 1890.)

As far as I am informed, the attempt to photograph the fixed stars by their own light has been made nowhere else, up to the present date; the rumor of a daguerreotype of a nebula made in Italy some years since was unfounded.

About seven years since (July 17, 1850) Mr. *Whipple* obtained daguerreotype impressions from the image of *α Lyræ* formed in the focus of the Great Equatorial, and subsequently from *Castor*, thus establishing a simple but not uninteresting fact—the possibility of such an achievement. On these occasions a long exposure of one or two minutes was required before the plate was acted upon by the light, and in this interval the irregularities of the Munich clock-work were so great as to destroy the symmetry of the images, while the smaller stars of the second magnitude would not "take" at all.

For some years after, Mr. *Whipple* gave his attention to photographs of the moon and sun, and the stars were left to themselves. But improvements in the art progressed rapidly; the preparations were more sensitive; the artists had acquired more experience. At the same time the principle of the spring governor had been thoroughly tested, and found to supply a great desideratum in imparting a sidereal motion to the telescope, incomparably more uniform than that attained by the Munich mechanism. Messrs. *Whipple* and *Black* recommenced their trials on star images (taken by the collodion process) in March of the present year, and they are still in progress. The expense of time, chemicals, etc., is far

[1] George Phillips Bond (1825–1865), the second director of the Harvard Observatory and the son of the first director, William Cranch Bond, devoted his short astronomical career mainly to studies of the great comet of 1858 (Donati's) and to the Orion Nebula. His pioneer work in stellar photography, summarized in the present article, shows that he realized that the substitution of permanent photographic records for transitory human visual impressions would be revolutionary in astronomical science.

more considerable than one would have anticipated—each night, in fact, opens new vistas requiring exploration. The field for experiment is too vast to be at once occupied, even if we were provided with unlimited means. But the results already obtained in the disconnected attempts we have thus far been enabled to make are of the highest interest, and suggest possibilities in the future which one can scarcely trust himself to speculate upon. Could another step in advance be taken equal to that gained since 1850, the consequences could not fail of being of incalculable importance in Astronomy.

The same object, *α Lyrae*, which in 1850 required 100 seconds to impart its image to the plate, and even then imperfectly, is now photographed instantaneously with a symmetrical disc, perfectly fit for exact micrometer measurements. We then were confined to a dozen or two of the brightest stars, whereas now we take all that are visible to the naked eye. Even from week to week we can distinguish decided progress.

Of the beauty and convenience of the method you will scarcely form a correct idea without witnessing for yourself, which I hope you will be able to do before long.

On a fine night the amount of work which can be accomplished, with entire exemption from the trouble, vexation and fatigue that seldom fail to attend upon ordinary observations, is astonishing. The plates, once secured, can be laid by for future study by daylight and at leisure. The record is there, with no room for doubt or mistake as to its fidelity. As yet, however, we obtain images only from stars to the sixth magnitude inclusive. To be of essential service to Astronomy, it is indispensable that great improvements be yet made, and these I feel sure will not be accomplished without a deal of experimenting.

But could we but press the matter on, we should soon be able to say what we can and what we cannot accomplish in stellar photography. The latter limits we certainly have not yet reached. At present the chief object of attention must be to improve the sensitiveness of the plates, to which, I am assured by high authorities in chemistry, there is scarcely any limit to be put in point of theory. Suppose we are able finally to obtain pictures of seventh-magnitude stars. It is reasonable to suppose that, on *some lofty mountain and in a purer atmosphere*, we might, with the same telescope, include the eighth magnitude. To increase the size of the telescope three-fold in aperture is a practicable thing, if the

money can be found. This would increase the brightness of the stellar images, say, eight-fold, and we should be able then to photograph all the stars to the tenth and eleventh magnitude inclusive. There is nothing, then, so extravagant in predicting a future application of photography to stellar Astronomy on a most magnificent scale. It is, even at this moment, simply a question of finding one or two hundred thousand dollars to make the telescope with and to keep up the experiments.

What more admirable method can be imagined for the study of the orbits of the fixed stars and for resolving the problem of their annual parallax than this would be, if we could obtain the impressions of the telescopic stars to the tenth magnitude? Consider, too, that groups of ten, or fifty even, if so many occur in the compass of the field, will be taken as quickly as one alone would be—perhaps in a few seconds only—and each mapped out with unimpeachable accuracy.

I have not alluded to two important features in stellar photography. One is that the intensity and size of the images, taken in connection with the length of time during which the plate has been exposed, measures the relative magnitudes of the stars. The other point is that the measurements of distances and angles of position of the double stars from the plates we have ascertained, by many trials on our earliest impressions, to be as exact as the best micrometric work. Our subsequent pictures are much more perfect, and should do better still.

CARRINGTON[1]

Discovery of Systematic Motions of Sun Spots

(From *Monthly Notices* of the Royal Astronomical Society, Vol. 19, 1858.)

I. Distribution of the Solar Spots in Latitude.—In the briefest form of statement, the result is, that throughout the two years preceding the minimum of frequency in February, 1856, the spots were confined to an equatoreal belt, and in no instance passed the limits of 20° of latitude N. or S.; and that shortly after this epoch, whether connected with it or not, this equatoreal series appears to have become extinct, and in seeming contradiction to the precept, *Natura non agit per saltum*, two new belts of disturbance abruptly commenced, the limits of which in both hemispheres may be roughly set at between 20° and 40°, with exceptions in favour of the old equatoreal region. The tendency at the present time appears to be to contraction of the parallels . . .

The variation of the limiting parallels being established, the inquirer next desires to turn back to such past records as may throw light on their changes and their relation to epochs. In this respect I would call attention to a short, but very condensed and important paper, communicated to me by Dr. C. H. F. Peters, formerly of Naples, and lately of Albany, and published, I believe, by the American Association for the Advancement of Science. Dr. Peters observed the sun with an object and purpose very similar to my own, from September, 1845 to October, 1846, inclusive, and obtained 813 places of 286 spots, which he subsequently reduced with a skill and exactness in which I place great confidence. He has given a plate exhibiting the gross distribution in latitude during the period of his labours, in which I find the limits laid down as 40° N. and 30° S. with a desolate region from 8° N. to 5° S., and with, on the whole, a preponderance of action in the north hemisphere. We know now that at that time the sun was passing from a period of minimum activity to a maximum, as now. The distribution is

[1] Richard Christopher Carrington (1826–1875), English observer, in a private observatory at Red Hill, studied the positions and movements of sun spots and catalogued the circumpolar stars.

very similar to that which now holds, excepting that at the present time there appears an excess of activity in the south. The records of Dr. Peters are not all which leisure and research may make available in this branch of the history of solar action, there are those of Soemmering and others; nevertheless, it is difficult to express the degree of regret which a student of the sun feels when evidence such as the present meets him of the state of maturity his subject might have attained ere this, had not the opportunities of two centuries been neglected, by his predecessors condemning the research as one of idle curiosity, fit matter for a University thesis, but below the level of Philosophy.

The most cursory consideration will show that success in educing such conclusions in the case of the sun depends mainly on the continuity of the labours of the observer. The conclusion which an observer would have arrived at from a discussion of the observations made during the years 1854 and 1855 would have been exceedingly imperfect, though apparently borne out by a tolerably extensive experience; and the conclusion which I draw from the four years' results now accumulated is, that our knowledge of the sun's action is but fragmentary, and that the publication of speculations on the nature of his spots would be a very precarious venture.

II. Equatorial Acceleration of the Sun's Rotation.—The present short paper is in continuation of a preceding one inserted in the number for November, my object being to illustrate . . . the divergence of individual spots in groups of small extent, and to establish clearly the circumstance that instances occur of change of position of detached spots, not explicable by change of form, but due to drift on the photosphere. It will then be seen that in any investigation of the elements of rotation of the sun, the movements observed in groups must be excluded as liable to special perturbations; that the discordances of the periods of rotation of the photosphere found by many previous investigators are in great measure due to the omission of the consideration of the latitude of the spots discussed, and that we have arrived at some grounds for the recognition of currents on the sun. I am compelled thus to treat the subject by portions from want of leisure at the present time to enter upon it more completely . . .

I have acknowledged before that Dr. C. H. F. Peters, now Professor of Astronomy at Hamilton College in the United States, has the right to claim priority of publication of this action upon

one another of the individuals of a group of spots, though, as the examples now given will show, it could not have escaped me in the examination of my own records. There are two ways in which this action may be mechanically explained; firstly, by the centre of eruption beneath the photosphere being situated between the openings in the photosphere which we see as dark spots, and its emissions of gaseous or other expansive matter being thrown out obliquely, an explanation which is strongly supported by instances occasionally occurring in which an elliptical group of numerous small nuclei have all their penumbræ turned (or thrown) outwards; secondly, by the pressure of the necessarily superincumbent matter of the photosphere thrown out of the cavity of the spots, and which, as the emissive force dies out, must, in its efforts to resume its general level, exert a lateral pressure. The mechanics of the subject may, however, wait awhile till observation has further advanced; my object at present being chiefly to show that groups present peculiarities of internal commotion which unfit them for discussion in the correction of the elements of rotation . . .

I have proceeded to select out of my stock of results all series of observed positions of single spots recorded on more than two days, to which no *a priori* objection existed, and to deduce for each series the mean daily drift of the spot in longitude and latitude, expressed in minutes of arc on the surface of the sun, and arranged the results in order of the latitude of the spots from north to south. As I do not consider the subject fully ripe, I will only now give the mean results of contiguous values, remarking that the groups have been made as usual with reference to the tendency exhibited to change of sign, and that the signs within the limits of the several groups presented a remarkable consistency. The resulting means are as follow:

Latitude	Daily drift		
	In longitude	In latitude	Series
30° N.	−25′−	+5′−	4
18 N.	−14	+1	4
8 N.	+ 8	−5	14
11 S.	+10	−3	6
19 S.	−10	+1	5
29 S.	−21	+4	8

There is a mean excess of retrogradation in longitude of 9′ a day with the period 25.380, which was employed provisionally in reduction. If we correct this period for the 9′, we have 14°2′ for the daily sidereal rotation, and $25^{d}.652$ for the corresponding period; but though this is the mean period which results, and happens to agree exactly with the mean period of Dr. C. H. F. Peters, it necessarily is affected by the mean drift, and may be not a whit nearer the truth than the one provisionally used, from the convenience of its ready subdivision. The numbers exhibited in the foregoing little table are however very significant, when the method of their derivation is duly borne in mind. They show that with a mean period of rotation there is an equatoreal current causing spots to move in the direction of the solar rotation, and a reverse current in the higher latitudes north and south, between 15° and 40°, causing the spots of those latitudes to apparently regress. It now appears at once, when the disclosure of my former map, in the November *Monthly Notice*, is remembered, how former observers may well have disagreed in their concluded periods, from having observed at different epochs when the limiting parallels were different in each case, and when the considerations of latitude and drift were omitted. It further results from the above table that between the parallels of 15° N. and S., the mean drift in latitude is towards the equator, and beyond 15° of latitude or thereabouts towards the poles in each hemisphere, and that the differences of drift in longitude are far larger than those in latitude. The effect of the mean drift in longitude is such that had I inferred a mean period of sidereal rotation from the observations of 1854 and 1855, there would have resulted $25^{d}.11\pm$, and from the subsequent period after the change of distribution exhibited in my map, a period of $25^{d}.90\pm$. A question may hence arise whether at the epoch of that striking change of distribution in latitude, a simultaneous change of currents did not set in, but our records are not yet sufficient to supply materials for the discussion of such speculations.

MAXWELL[1]

The Nature of Saturn's Rings

(From "On the Stability of the Motion of Saturn's Rings," London, 1859.)

Statement of the Problem.—There are some questions in Astronomy, to which we are attracted rather on account of their peculiarity, as the possible illustration of some unknown principle, than from any direct advantage which their solution would afford to mankind. The theory of the Moon's inequalities, though in its first stages it presents theorems interesting to all students of mechanics, has been pursued into such intricacies of calculation as can be followed up only by those who can make the improvement of the Lunar Tables the object of their lives. The value of the labours of these men is recognized by all who are aware of the importance of such tables in Practical Astronomy and Navigation. The methods by which the results are obtained are admitted to be sound, and we leave to professional astronomers the labour and the merit of developing them.

The questions which are suggested by the appearance of Saturn's Rings cannot, in the present state of Astronomy, call forth so great an amount of labour among mathematicians. I am not aware that any practical use has been made of Saturn's Rings, either in Astronomy or in Navigation. They are too distant, and too insignificant in mass, to produce any appreciable effect on the motion of other parts of the Solar system; and for this very reason it is difficult to determine those elements of their motion which we obtain so accurately in the case of bodies of greater mechanical importance.

But when we contemplate the Rings from a purely scientific point of view, they become the most remarkable bodies in the heavens, except, perhaps, those still less *useful* bodies—the spiral nebulæ. When we have actually seen that great arch swung over the equator of the planet without any visible connection,

[1] James Clerk Maxwell (1831–1879), English mathematical physicist, obtained the Adams prize in the year 1856 for his essay "On the Stability of the Motion of Saturn's Rings."

we cannot bring our minds to rest. We cannot simply admit that such is the case, and describe it as one of the observed facts in nature, not admitting or requiring explanation. We must either explain its motion on the principles of mechanics, or admit that, in the Saturnian realms, there can be motion regulated by laws which we are unable to explain . . .

We know, since it has been demonstrated by Laplace, that a uniform solid ring cannot revolve permanently about a planet. We propose in this Essay to determine the amount and nature of the irregularity which would be required to make a permanent rotation possible. We shall find that the stability of the motion of the ring would be ensured by loading the ring at one point with a heavy satellite about four and one-half times the weight of the ring, but this load, besides being inconsistent with the observed appearance of the rings, must be far too artificially adjusted to agree with the natural arrangements observed elsewhere, for a very small error in excess or defect would render the ring again unstable.

We are, therefore, constrained to abandon the theory of a solid ring, and to consider the case of a ring, the parts of which are not rigidly connected, as in the case of a ring of independent satellites, or a fluid ring.

There is now no danger of the whole ring or any part of it being precipitated on the body of the planet. Every particle of the ring is now to be regarded as a satellite of Saturn,[1] disturbed by the attraction of a ring of satellites at the same mean distance from the planet, each of which however is subject to slight displacements. The mutual action of the parts of the ring will be so small compared with the attraction of the planet, that no part of the ring can ever cease to move round Saturn as a satellite.

But the question now before us is altogether different from that relating to the solid ring. We have now to take account of variations in the form and arrangement of the parts of the ring, as well as its motion as a whole, and we have as yet no security that these variations may not accumulate till the ring entirely loses its original form, and collapses into one or more satellites, circulating round Saturn. In fact, such a result is one of the leading doctrines of the "nebular theory" of the formation of planetary

[1 Compare this deduction with the speculation concerning Saturn's rings in the concluding paragraph of the quotation on page 116, from Thomas Wright.]

systems: and we are familiar with the actual breaking up of fluid rings under the action of "capillary" force, in the beautiful experiments of M. Plateau.

In this essay I have shewn that such a destructive tendency actually exists, but that by the revolution of the ring it is converted into the condition of dynamical stability. As the scientific interest of Saturn's Rings depends at present mainly on this question of their stability, I have considered their motion rather as an illustration of general principles, than as a subject for elaborate calculation, and, therefore, I have confined myself to those parts of the subject which bear upon the question of the permanence of a given form of motion.

There is a very general and very important problem in Dynamics, the solution of which would contain all the results of this Essay and a great deal more. It is this: "Having found a particular solution of the equations of motion of any material system, to determine whether a slight disturbance of the motion indicated by the solution would cause a small periodic variation, or a total derangement of the motion."

The question may be made to depend upon the conditions of a maximum or a minimum of a function of many variables, but the theory of the tests for distinguishing maxima from minima by the Calculus of Variations becomes so intricate when applied to functions of several variables, that I think it doubtful whether the physical or the abstract problem will be first solved.

[With this introduction the author proceeds with a mathematical discussion, extending through sixty quarto pages, and the results are summarized in the following paragraphs.]

Summary of Conclusions.—Let us now gather together the conclusions we have been able to draw from the mathematical theory of various kinds of conceivable rings.

We found that the stability of the motion of a solid ring depended on so delicate an adjustment, and at the same time so unsymmetrical a distribution of mass, that even if the exact condition were fulfilled, it could scarcely last long, and if it did, the immense preponderance of one side of the ring would be easily observed, contrary to experience. These considerations, with others derived from the mechanical structure of so vast a body, compel us to abandon any theory of solid rings.

We next examined the motion of a ring of equal satellites, and found that if the mass of the planet is sufficient, any disturbances

produced in the arrangement of the ring will be propagated round it in the form of waves, and will not introduce dangerous confusion. If the satellites are unequal, the propagation of the waves will no longer be regular, but disturbances of the ring will in this, as in the former case, produce only waves, and not growing confusion. Supposing the ring to consist, not of a single row of large satellites, but of a cloud of evenly distributed unconnected particles, we found that such a cloud must have a very small density in order to be permanent, and that this is inconsistent with its outer and inner parts moving with the same angular velocity. Supposing the ring to be fluid and continuous, we found that it will be necessarily broken up into small portions.

We conclude, therefore, that the rings must consist of disconnected particles; these may be either solid or liquid, but they must be independent. The entire system of rings must therefore consist either of a series of many concentric rings, each moving with its own velocity, and having its own systems of waves, or else of a confused multitude of revolving particles, not arranged in rings, and continually coming into collision with each other.

Taking the first case, we found that in an indefinite number of possible cases the mutual perturbations of two rings, stable in themselves, might mount up in time to a destructive magnitude, and that such cases must continually occur in an extensive system like that of Saturn, the only retarding cause being the possible irregularity of the rings.

The result of long-continued disturbance was found to be the spreading out of the rings in breadth, the outer rings pressing outwards, while the inner rings press inwards.

The final result, therefore, of the mechanical theory is, that the only system of rings which can exist is one composed of an indefinite number of unconnected particles, revolving round the planet with different velocities according to their respective distances. These particles may be arranged in series of narrow rings, or they may move through each other irregularly. In the first case, the destruction of the system will be very slow, in the second case, it will be more rapid, but there may be a tendency towards an arrangement in narrow rings, which may retard the process.

We are not able to ascertain by observation the constitution of the two outer divisions of the system of rings, but the inner ring is certainly transparent, for the limb of Saturn has been observed through it. It is also certain, that though the space occupied by

the ring is transparent, it is not through the material parts of it that Saturn was seen, for his limb was observed without distortion; which shows that there was no refraction, and, therefore, that the rays did not pass through a medium at all, but between the solid or liquid particles of which the ring is composed. Here then we have an optical argument in favour of the theory of independent particles as the material of the rings. The two outer rings may be of the same nature, but not so exceedingly rare that a ray of light can pass through their whole thickness without encountering one of the particles.

Finally, the two outer rings have been observed for 200 years, and it appears, from the careful analysis of all the observations by Struve, that the second ring is broader than when first observed, and that its inner edge is nearer the planet than formerly. The inner ring also is suspected to be approaching the planet ever since its discovery in 1850. These appearances seem to indicate the same slow progress of the rings towards separation which we found to be the result of theory, and the remark, that the inner edge of the inner ring is most distinct, seems to indicate that the approach towards the planet is less rapid near the edge, as we had reason to conjecture. As to the apparent unchangeableness of the exterior diameter of the outer ring, we must remember that the outer rings are certainly far more dense than the inner one, and that a small change in the outer rings must balance a great change in the inner one. It is possible, however, that some of the observed changes may be due to the existence of a resisting medium. If the changes already suspected should be confirmed by repeated observations with the same instruments, it will be worth while to investigate more carefully whether Saturn's Rings are permanent or transitionary elements of the Solar System, and whether in that part of the heavens we see celestial immutability, or terrestrial corruption and generation, and the old order giving place to new before our own eyes.

KIRCHHOFF[1]

The Absorption Spectrum of the Sun

(From "Researches on the Solar Spectrum and the Spectra of the Chemical Elements," 1861; translated by Henry E. Roscoe, 1862.)

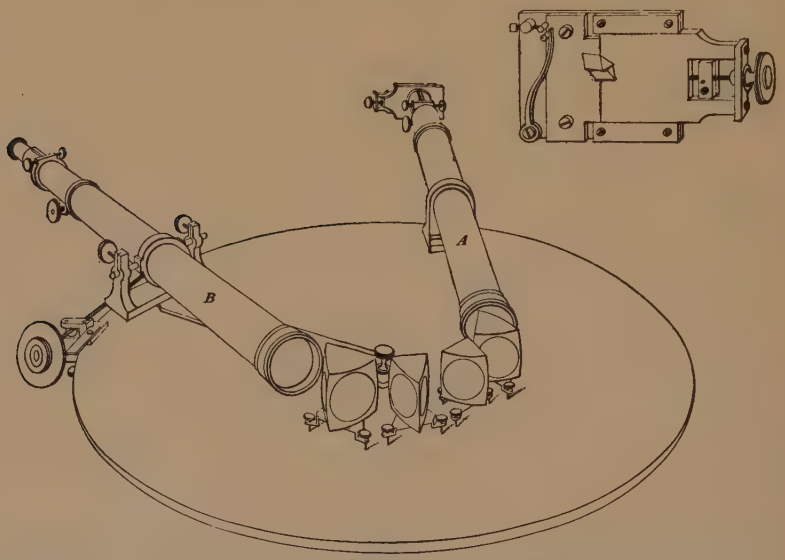
In the course of the experiments already alluded to, which Foucault instituted on the spectrum of the electric arc formed between the carbon points, this physicist observed that the bright sodium lines present were changed into dark bands in the spectrum produced by the light from one of the carbon poles, which had been allowed to pass through the luminous arc; and when he passed direct sunlight through the arc he noticed that the double *D* line was seen with an unusual degree of distinctness. No attempt was made to explain or to increase these observations either by Foucault or by any other physicist, and they remained unnoticed by the greatest number of experimentalists. They were unknown to me when Bunsen and I, in the year 1859, commenced our investigations on the spectra of coloured flames.

In order to test in the most direct manner possible the truth of the frequently asserted fact of the coincidence of the sodium lines with the lines *D*, I obtained a tolerably bright solar spectrum, and brought a flame coloured by sodium vapour in front of the slit. I then saw the dark lines *D* change into bright ones. The flame of a Bunsen's lamp threw the bright sodium lines upon the solar spectrum with unexpected brilliancy. In order to find out the extent to which the intensity of the solar spectrum could be increased, without impairing the distinctness of the sodium lines, I allowed the full sunlight to shine through the sodium flame upon the slit, and, to my astonishment, I saw that the dark lines *D* appeared with an extraordinary degree of clearness. I then exchanged the sunlight for the Drummond's or oxyhydrogen lime-light, which, like that of all incandescent solid or liquid bodies, gives a spectrum containing no dark lines. When this light was allowed to fall through a suitable flame coloured by common salt,

[1] Gustav Robert Kirchhoff (1824–1887), German physicist, established the science of spectrum analysis and applied it to celestial objects.

dark lines were seen in the spectrum in the position of the sodium lines. The same phenomenon was observed if instead of the incandescent lime a platinum wire was used, which being heated in a flame was brought to a temperature near to its melting point by passing an electric current through it.

The phenomenon in question is easily explained upon the supposition that the sodium flame absorbs rays of the same degree of refrangibility as those it emits, whilst it is perfectly transparent for all other rays. This supposition is rendered probable by the



The apparatus employed by Kirchhoff for the observations of the Solar spectrum.

fact, which has long been known, that certain gases, as for instance, nitrous acid and iodine vapour, possess at low temperatures the property of such a selective absorption. The following considerations shew that this is the true explanation of the phenomenon. If a sodium flame be held before an incandescent platinum wire whose spectrum is being examined, the brightness of the light in the neighbourhood of the sodium lines would, according to the above supposition, *not* be altered; in the position of the sodium lines themselves, however, the brightness *is* altered, for two reasons; in the first place, the intensity of light emitted by the platinum wire is reduced to a certain fraction of its original amount by absorption in the flame, and secondly, the light of the flame itself is added to

that from the wire. It is plain that if the platinum wire emits a sufficient amount of light, the loss of light occasioned by absorption in the flame must be greater than the gain of light from the luminosity of the flame; the sodium lines must then appear darker than the surrounding parts, and by contrast with the neighbouring parts they may seem to be quite black, although their degree of luminosity is necessarily greater than that which the sodium flame alone would have produced.

The absorptive power of sodium vapour becomes most apparent when its luminosity is smallest, or when its temperature is lowest. In fact we were unable to produce the dark sodium lines in the spectrum of a Drummond's light, or in that of an incandescent wire, by means of a Bunsen's gas-flame in which common salt was placed; but the experiment succeeded with a flame of aqueous alcohol containing common salt. The following experiment proposed by Crookes likewise very clearly shews this influence of temperature. If a piece of sodium is burnt in a room, and the air thus filled with the vapour of sodium compounds, every flame is seen to burn with the characteristic yellow light. If a small flame in which a bead of soda salt is placed be now fixed in front of a large one, so that the former is seen projected on the latter as a background, the small flame appears to be surrounded with a black smoky mantle. This dark mantle is produced by the absorptive action of the sodium vapours in the outer part of the flame, which are cooler than those in the flame itself. Bunsen and I have produced the dark lines in the spectrum of a common candle-flame, by allowing the rays to pass through a test tube containing a small quantity of sodium-amalgam, which we heated to boiling; so that the sodium vapour effecting the absorption had in this case possessed a temperature far below the red-heat. The same phenomenon is observed in a much more striking manner if a glass tube is used containing some small pieces of sodium first filled with hydrogen, and then rendered vacuous and sealed. The lower end of the tube can be heated so as to vaporize the sodium. By means of this arrangement, which was proposed by Roscoe, the heated vapour of the sodium, when viewed by the sodium-light, is seen as a dark black smoke which throws a deep shadow, but is perfectly invisible when observed by the ordinary gas-light . . .

The sodium flame is characterized beyond that of any other coloured flame by the intensity of the lines in its spectrum. Next

to it in this respect comes the lithium flame. It is just as easy to reverse the red lithium line, that is, to turn the bright line into a dark one, as it is to reverse the sodium line. If direct sunlight be allowed to pass through a lithium flame, the spectrum exhibits in the place of the red lithium band a black line which in distinctness bears comparison with the most remarkable of Fraunhofer's lines, and disappears when the flame is withdrawn. It is not so easy to obtain the reversal of the spectra of the other metals; nevertheless Bunsen and I have succeeded in reversing the brightest lines of potassium, strontium, calcium, and barium, by exploding mixtures of the chlorates of these metals and milk-sugar in front of the slit of our apparatus whilst the direct solar rays fell on the instrument.

These facts would appear to justify the supposition that each incandescent gas diminishes by absorption the intensity of those rays only which possess degrees of refrangibility equal to those of the rays which it emits; or, in other words, that the spectrum of every incandescent gas must be reversed, when it is penetrated by the rays of a source of light of sufficient intensity giving a continuous spectrum.

FOUCAULT[1]

Laboratory Determination of the Speed of Light, and the Distance to the Sun

(From *Comptes Rendus* de l'Académie des Sciences, Vol. 55, 1862.)

At the meeting of May 6, 1850, I gave the results of a differential experiment on the speed of light in two media of unequal densities; and at the same time I pointed out that the same procedure, based on the use of the rotating mirror, would later serve to measure the absolute speed of light in space.

After this plan was seriously discussed, the Director of the Observatory wished to hasten its execution, and put at my disposal the necessary resources. At the beginning of the summer the apparatus was in working order, but bad weather did not permit me to devote myself as promptly as I might have desired to the observations which required the presence of sun-light. However, the sky finally cleared, and profiting by these recent fine days I have obtained results which seem to me to contain something near the truth.

The present apparatus differs essentially from that which has been previously described, only in the addition of clockwork designed to move a circular diaphragm cogged for the accurate measurement of the speed of the mirror, and in the extension of the experimental line, which, by means of multiple reflections, has been carried from 4 to 20 meters. Increasing in this manner the length of the luminous path and obtaining higher precision in the measure of the time, I have obtained determinations of which the extreme variations do not exceed one part in a hundred and which, combined in groups, readily give values which agree to one part in five hundred.

Definitively, the speed of light is notably diminished. According to previous data, the speed would be 308 million meters per

[1] Jean Bernard Léon Foucault (1819–1868), French physicist and astronomer, measured the velocity of light by a laboratory method, inaugurated silvered glass for mirrors in large telescopes, and is widely known for his demonstration (with the Foucault pendulum) of the earth's rotation

second, and the new experiment with the rotating mirror gives, in round numbers, 298 millions.

It seems to me that one can count on the accuracy of this number, in the sense that the corrections which it might undergo should not exceed 500,000 meters.

If one accepts this new value and combines it with the constant of aberration 20″.45 in order to deduct from it the Sun's parallax,[1] which is obviously a function of both, one finds, instead of 8″.59, the notably greater value, 8″.86. Thus the mean distance from the Earth to the Sun is diminished about one thirtieth.

To give an idea of the degree of confidence that can be accorded to the plan of observation which has been followed in the present experiment, I shall transcribe here a series of rough determinations chosen from those for which the mean agrees the best with the general mean.

1024	1026	1026	1028	
1025	1026	1027	1028	
1029	1026	1025	1027	
1028	1025	1026	1026.5	
1027	1026	1027	1027	Mean 1026.47

[1] The distance in miles d is related to the parallax in seconds of arc p by the equation $d = 8 \times 10^8/p$.]

HERBERT SPENCER[1]

Cosmic Evolution

(From "First Principles," 6th Ed.,[2] 1900.)

The stars are distributed with a three-fold irregularity. There is first the marked contrast between the Milky Way and other parts of the heavens, in respect to the quantities of stars within given visual areas. There are secondary contrasts of like kind in the Milky Way itself, which has its thick and thin places; as well as throughout the celestial spaces in general, which are more closely strewn in some regions than in others. And there is a third order of contrasts produced by the aggregation of stars into small clusters. Besides this heterogeneity in the distribution of stars, considered without distinctions of kind, a further heterogeneity is disclosed when they are classified by their differences of colour, which answer to differences of physical constitution. While yellow stars are found in all parts of the heavens, red and blue stars are not so: there are wide regions in which both red and blue stars are rare; there are regions in which the blue occur in considerable numbers, and there are other regions in which the red are comparatively abundant. Yet one more irregularity of like significance is presented by the nebulæ. These are not dispersed with anything like uniformity, but are far more numerous around the poles of the galactic circle than in the neighbourhood of its plane.

No one will expect that anything like a definite interpretation of this structure can be given on the hypothesis of Evolution, or any other hypothesis. Such an interpretation would imply some reasonable assumption respecting the pre-existing distribution of the stellar matter and of the matter forming nebulæ, and we have no warrant for any assumption. If we allow imagination

[1] Herbert Spencer (1820–1903), English philosopher, included in his attempted general synthesis of scientific knowledge a survey of the problems of universal evolution; his speculations have been of wide influence in scientific philosophy, but the advances of observational astronomy have naturally shattered many of his specific hypotheses.

[2] First edition, 1862.]

to range back through antecedent possibilities and probabilities, we see it to be unlikely that homogeneous matter filled the space which our Sidereal System now fills, at a time immediately preceding its initiation. Rather the evidence which the heavens present implies that the distribution out of which present distribution arose was irregular in all respects. Though certain traits of our galaxy suggest that it has a vague individuality, and that, along with their special motions, its stars have some general motion; yet the evidence forces on us the conclusion that many varieties of changes have been simultaneously going on in its different parts. We find nebulæ in all stages of concentration, star clusters variously condensed, groups of larger stars approximating in different degrees, as well as regions like those which the nubeculæ occupy, presenting complex structures and apparently active changes. The most which can be said respecting this total distribution is that, subject as all parts of our Sidereal System are to the law of gravitation, the heterogeneities it exhibits, everywhere implying a progressing concentration, that is, integration, point backward to a less heterogeneous state and point forward to a more heterogeneous state. But, leaving aside this too transcendent question, we may without undue rashness consider from the evolution point of view the changes to be anticipated in one of those collections of matter described as a diffused nebulosity, or one of those more distinct ones of which the outlying parts are compared to wisps of cloud blown about by the wind. The only evolutional process which can at first be displayed is the primary one of integration—the gathering together through mutual attraction of the parts; for in this early stage in which indefiniteness and incoherence are so fully exemplified, there does not yet exist such an aggregate as is capable of exhibiting secondary re-distributions: we have only the dispersed components of such an aggregate. Contemplating, then, only the process of integration, we may, without asking anything about the previous history of an irregular nebula, safely assume that its parts have their respective proper motions; for the chances are infinity to one against a state of rest relatively to one another. Further, the chances are infinity to one against their proper motions being such that during concentration they will cancel one another: the motion of some part, or the resultant of the motions of several parts, will constitute a proper motion distinct from that which mutual gravitation generates—a motion which, unless just counterbalanced by an

opposite one (again an infinite improbability) will generate rotation. It may, indeed, be argued that, apart from any pre-existing proper motions of its parts, a nebulous mass, if irregular, will acquire rotation while integrating; since each outlying fragment, arriving after the rest have been gathered together, is infinitely unlikely to fall into the mass in such a manner that its motion will be entirely cancelled by resistance; but, falling into it so as to be deflected laterally, will have its motion of approach so changed in direction as to become in part a motion of revolution: a resultant of all such motions, largely conflicting, being an eventual rotation of the mass. It must not, however, be assumed that this will necessarily be the rotation of a solitary aggregate. The great nebula in *Andromeda* does not appear on the way to form a single body; and that in *Canes Venatici* is an advanced spiral of which the outer parts have a tangential motion too great to permit of their being drawn into the centre. Rather the apparent implication of the structure is that there will be formed a cluster of masses revolving round a common centre of gravity. Such cases, joined with those of the annular nebulæ, suggest that often the processes of integration result in compound structures, various in their kinds, while in other cases, and perhaps most frequently, single masses of rotating nebulous matter are formed.

Ignoring all such possibilities and probabilities, however, and limiting our attention to that form of the nebular hypothesis which regards the solar system as having resulted from a rotating spheroid of diffused substance; let us consider what consequence the instability of the homogeneous necessitates. Being oblate in figure, unlike in the densities of its centre and surface, unlike in their temperatures, and probably unlike in the angular velocities of its parts, such a mass cannot be called homogeneous; and any further changes exhibited by it can illustrate the general law, only as being changes from a more homogeneous to a less homogeneous state. Just noting that one of these changes is the increasing oblateness of form, let us go on to observe those which are to be found in the transformations of such of its parts as are at first homogeneous within themselves. If we accept the conclusion that the equatorial portion of this rotating and contracting spheroid will, at successive stages, have a centrifugal force great enough to prevent nearer approach to the centre of rotation, and will so be left behind; we shall find, in the fate of the detached ring, an exemplification of the

principle we are following out. Consisting of gaseous matter, such a ring, even if uniform at the time of its detachment could not continue so. In the absence of equality among the forces, internal and external, acting on it, there must be a point or points at which the cohesion of its parts would be less than elsewhere—a point or points at which rupture would therefore take place. The original assumption was that the ring would rupture at one place only, and would then collapse on itself. But this was a more than questionable assumption: such, at least, I know to have been the opinion of the late Sir John Herschel. So vast a ring, consisting of matter having such feeble cohesion, must break up into many parts. Nevertheless, appeal to another high authority—the late Sir G. B. Airy—yielded verification for the belief that the ultimate result which Laplace predicted would take place. And here is furnished a further illustration of the instability of the homogeneous. For even supposing the masses of nebulous matter into which such a ring is separated, were so much alike in their sizes and distances as to attract one another with exactly equal forces (which is infinitely improbable); yet the unequal actions of external disturbing forces would inevitably destroy their equilibrium—there would be one or more points at which adjacent masses would begin to part company. Separation, once commenced, would with accelerating speed lead to a grouping of the masses. A like result would eventually take place with the groups thus formed; until they at length aggregated into a single mass.

Already so many references have been made to the formation of a crust over the originally incandescent Earth, that it may be thought superfluous again to name it. It has not, however, been thus far considered in connexion with the general principle under discussion. Here it must be noted as a necessary consequence of the instability of the homogeneous. In this cooling and solidification of the Earth's surface, we have one of the simplest, as well as one of the most important, instances of that change from a uniform to a multiform state which occurs in any mass through exposure of its component parts to unlike conditions. To the differentiation of the Earth's exterior from its interior, thus brought about, we must add one of the most conspicuous differentiations which the exterior itself afterwards undergoes, as being similarly brought about. Were the forces to which the surface of the Earth is subject alike in all directions, there would be no reason why certain of its parts should become permanently unlike the rest. But being

unequally exposed to the chief external centre of force—the Sun—its main divisions become unequally modified. While the crust thickens and cools, there arises that contrast, now so decided, between the polar and equatorial regions.

SIR WILLIAM HUGGINS[1]

Spectra of Nebulæ

(From *The Nineteenth Century Review*, June, 1897.)

Soon after the completion of the joint work of Dr. Miller and myself, and then working alone, I was fortunate in the early autumn of the same year, 1864, to begin some observations in a region hitherto unexplored; and which, to this day, remain associated in my memory with the profound awe which I felt on looking for the first time at that which no eye of man had seen, and which even the scientific imagination could not foreshow.

The attempt seemed almost hopeless. For not only are the nebulæ very faintly luminous—as Marius put it, "like a rush-light shining through a horn"—but their feeble shining cannot be increased in brightness, as can be that of the stars, neither to the eye nor in the spectroscope, by any optic tube, however great.

Shortly after making the observations of which I am about to speak, I dined at Greenwich, Otto Struve being also a guest; when, on telling of my recent work on the nebulæ, Sir George Airy said: "It seems to me a case of 'Eyes and No Eyes.'" Such work indeed it was, as we shall see, on certain of the nebulæ.

The nature of these mysterious bodies was still an unread riddle. Towards the end of the last century the elder Herschel, from his observations at Slough, came very near suggesting what is doubtless the true nature, and place in the Cosmos, of the nebulæ. I will let him speak in his own words:

"A shining fluid of a nature unknown to us.

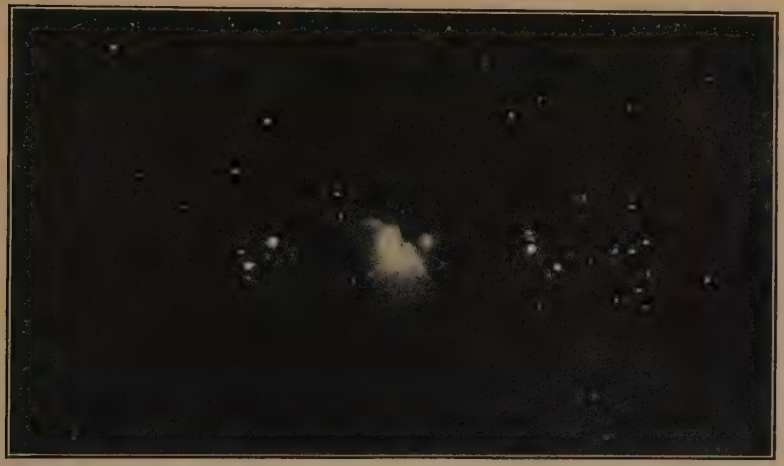
"What a field of novelty is here opened to our conceptions! . . . We may now explain that very extensive nebulosity, expanded over more than sixty degrees of the heavens, about the constellation of

[1] William Huggins (1824–1910), English astronomer, a pioneer in spectroscopy and photography. He examined spectroscopically the chemical constitution of stars and comets, and the gaseous nature of planetary and diffuse nebulæ; he applied the Doppler Principle to the measurement of the radial velocities of stars, and published an atlas of representative stellar spectra.

Orion; a luminous matter accounting much better for it than clustering stars at a distance . . .

"If this matter is self luminous, it seems more fit to produce a star by its condensation, than to depend on the star for its existence."

This view of the nebulæ as parts of a fiery mist out of which the heavens had been slowly fashioned, began, a little before the middle of the present century, at least in many minds, to give way before the revelations of the giant telescopes which had come into



The gaseous nebula in the Sword of Orion. From a Harvard Observatory photograph.

use, and especially of the telescope, six feet in diameter, constructed by the late Earl of Rosse at a cost of not less than £12,000.

Nebula after nebula yielded, being resolved apparently into innumerable stars, as the optical power was increased; and so the opinion began to gain ground that all nebulæ may be capable of resolution into stars. According to this view, nebulæ would have to be regarded, not as early stages of an evolutional progress, but rather as stellar galaxies already formed, external to our system—cosmical "sandheaps" too remote to be separated into their component stars. Lord Rosse himself was careful to point out that it would be unsafe from his observations to conclude that all nebulosity is but the glare of stars too remote to be resolved by our instruments. In 1858, Herbert Spencer showed clearly

that, notwithstanding the Parsonstown revelations, the evidence from the observation of nebulæ up to that time was really in favour of their being early stages of an evolutional progression.

On the evening of the 29th of August, 1864, I directed the telescope for the first time to a planetary nebula in Draco. The reader may now be able to picture to himself to some extent the feeling of excited suspense, mingled with a degree of awe, with which, after a few moments of hesitation, I put my eye to the spectroscope. Was I not about to look into a secret place of creation?

I looked into the spectroscope. No spectrum such as I expected! A single bright line only! At first, I suspected some displacement of the prism, and that I was looking at a reflection of the illuminated slit from one of its faces. This thought was scarcely more than momentary; then the true interpretation flashed upon me. The light of the nebula was monochromatic, and so, unlike any other light I had as yet subjected to prismatic examination, could not be extended out to form a complete spectrum. After passing through the two prisms it remained concentrated into a single bright line, having a width corresponding to the width of the slit, and occupying in the instrument a position at that part of the spectrum to which its light belongs in refrangibility. A little closer looking showed two other bright lines on the side towards the blue, all the three lines being separated by intervals relatively dark.

The riddle of the nebulæ was solved. The answer, which had come to us in the light itself, read: Not an aggregation of stars, but a luminous gas. Stars after the order of our own sun, and of the brighter stars, would give a different spectrum; the light of this nebula had clearly been emitted by a luminous gas. With an excess of caution, at the moment I did not venture to go further than to point out that we had here to do with bodies of an order quite different from that of the stars. Further observations soon convinced me that, though the short span of human life is far too minute relatively to cosmical events for us to expect to see in succession any distinct steps in so august a process, the probability is indeed overwhelming in favour of an evolution in the past, and still going on, of the heavenly hosts. A time surely existed when the matter now condensed into the sun and planets filled the whole space occupied by the solar system, in the condition of gas, which then appeared as a glowing nebula, after the order, it may be, of

some now existing in the heavens. There remained no room for doubt that the nebulæ, which our telescopes reveal to us, are the early stages of long processions of cosmical events, which correspond broadly to those required by the nebular hypothesis in one or other of its forms . . .

The solution of the primary riddle of the nebulæ left pending some secondary questions. What chemical substances are represented by the newly found bright lines? Is solar matter common to the nebulæ as well as to the stars? What are the physical conditions of the nebulous matter?

Further observations showed two lines of hydrogen; and recent observations have shown associated with it the new element recently discovered by Professor Ramsay, occluded in certain minerals, and of which a brilliant yellow line in the sun had long been looked upon as the badge of an element as yet unknown. The principal line of these nebulæ suggests probably another substance[1] which has not yet been unearthed from its hiding place in terrestrial rocks by the cunning of the chemist.

Are the nebulæ very hot, or comparatively cool? The spectroscope indicates a high temperature: that is to say, that the individual molecules or atoms, which by their encounters are luminous, have motions corresponding to a very high temperature, and in this sense are very hot. On account of the great extent of the nebulæ, however, a comparatively small number of luminous molecules might be sufficient to make them as bright as they appear to us: taking this view, their mean temperature, if they can be said to have one, might be low, and so correspond with what we might expect to find in gaseous masses at an early stage of condensation.

In the nebulæ I had as yet examined, the condensation of nearly all the light into a few bright lines made the observations of their spectra less difficult than I feared would be the case. It became, indeed, a case of "Eyes and No Eyes" when a few days later I turned the telescope to the Great Nebula in Andromeda. Its light was distributed throughout the spectrum, and consequently extremely faint. The brighter middle part only could be seen, though I have since proved, as I at first suggested might be the case, that the blue and the red ends are really not absent, but are not seen on account of their feebler effect upon the eye. Though

[1 Not until 1927 was it shown by I. S. Bowen that the unknown nebular lines, long attributed to nebulium, should be assigned to ionized oxygen and nitrogen.]

continuous, the spectrum did not look uniform in brightness, but its extreme feebleness made it uncertain whether the irregularities were due to certain parts being enhanced by bright lines, or the other parts enfeebled by dark lines.

Out of sixty of the brighter nebulæ and clusters, I found about one-third, including the planetary nebulæ and that of Orion, to give the bright-line spectrum . . .

Motion in the Line of Sight

From the beginning of our work upon the spectra of the stars, I saw in vision the application of the new knowledge to the creation of a great method of astronomical observation which could not fail in future to have a powerful influence on the progress of astronomy; indeed, in some respects greater than the more direct one of the investigation of the chemical nature and the relative physical conditions of the stars.

It was the opprobrium of the older astronomy—though indeed one which involved no disgrace, for *à l'impossible nul n'est tenu*—that only that part of the motions of the stars which is across the line of sight could be seen and directly measured. The direct observation of the other component in the line of sight, since it caused no change of place and, from the great distance of the stars, no appreciable change of size or of brightness within an observer's lifetime, seemed to lie hopelessly quite outside the limits of man's powers. Still, it was only too clear that, so long as we were unable to ascertain directly those components of the stars' motions which lie in the line of sight, the speed and direction of the solar motion in space, and many of the great problems of the constitution of the heavens, must remain more or less imperfectly known.

Now as the colour of a given kind of light, and the exact position it would take up in a spectrum, depends directly upon the length of the waves, or, to put it differently, upon the number of waves which would pass into the eye in a second of time, it seemed more than probable that motion between the source of the light and the observer must change the apparent length of the waves to him, and the number reaching his eye in a second. To a swimmer striking out from the shore each wave is shorter, and the number he goes through in a given time is greater than would be the case if he had stood still in the water. Such a change of wave length would transform any given kind of light, so that it would take a new place in the spectrum, and from the amount of this change to a

higher or to a lower place, we could determine the velocity per second of the relative motion between the star and the earth . . .

The discovery of the actual velocity of light was made by Roemer in 1675, from observations of the satellites of Jupiter. Now though the effect of motion in the line of sight upon the apparent velocity of light underlies Roemer's determinations, the idea of a change of colour in light from motion between the source of light and the observer was announced for the first time by Doppler in 1841. Later, various experiments were made in connection with this view by Ballot, Sestini, Klinkerfues, Clerk Maxwell, and Fizeau. But no attempts had been made, nor were indeed possible, to discover by this principle the motions of the heavenly bodies in the line of sight. For, to learn whether any change in the light had taken place from motion in the line of sight, it was clearly necessary to know the original wave length of the light before it left the star.

As soon as our observations had shown that certain earthly substances were present in the stars, the original wave lengths of their lines became known, and any small want of coincidence of the stellar lines with the same lines produced upon the earth might safely be interpreted as revealing the velocity of approach or of recession between the star and the earth.

These considerations were present to my mind from the first, and helped me to bear up under many toilsome disappointments: *Studio fallente laborem.* It was not until 1866 that I found time to construct a spectroscope of greater power for this research. It would be scarcely possible, even with greater space, to convey to the reader any true conception of the difficulties which presented themselves in this work, from various instrumental causes, and of the extreme care and caution which were needful to distinguish spurious instrumental shifts of a line from a true shift due to the star's motion.

At last, in 1868, I felt able to announce in a paper printed in the Transactions of the Royal Society for that year, the foundation of this new method of research, which, transcending the wildest dreams of an earlier time, enables the astronomer to measure off directly in terrestrial units the invisible motions in the line of sight of the heavenly bodies . . .

It has become fruitful in another direction, for it puts into our hands the power of separating double stars which are beyond the resolving power of any telescope that can ever be constructed.

Pickering and Vogel have independently discovered by this method an entirely new class of double stars.

Double stars too close to be separately visible unite in giving a compound spectrum. Now, if the stars are in motion about a common centre of gravity, the lines of one star will shift periodically relatively to similar lines of the other star, in the spectrum common to both; and such lines will consequently, at those times, appear double. Even if one of the stars is too dark to give a spectrum which can be seen upon that of the other star, as is actually the case with Algol and Spica,[1] the whirling of the stars about each other may be discovered from the periodical shifting of the lines of the brighter star relatively to terrestrial lines of the same substance. It is clear that as the stars revolve about their common centre of gravity, the bright star would be sometimes advancing, and at others receding, relatively to an observer on the earth, except it should so happen that the stars' orbit were perpendicular to the line of sight.

It would be scarcely possible, without the appearance of great exaggeration, to attempt to sketch out even in broad outline the many glorious achievements which doubtless lie before this method of research in the immediate future.

Spectra of Comets

Comets in the olden time were looked upon as the portents of all kinds of woe:

> There with long bloody haire, a blazing star
> Threatens the World with Famin, Plague, and War.

Though they were no longer, at the time of which I am speaking, a terror to mankind, they were a great mystery. Perhaps of no other phenomenon of nature had so many guesses at truth been made on different, and even on opposing principles of explanation. It was about this time that a beam of light was thrown in, for the first time, upon the night of mystery in which they moved and had their being, by the researches of Newton of Yale College, by Adams, and by Schiaparelli. The unexpected fact came out of the close relationship of the orbits of certain comets with those of periodic meteor swarms. Only a year before the observations of which I am about to speak were made, Odling had lighted up the theatre of the Royal Institution with gas brought by a meteorite

[1 Spica is now known to be composed of two stars nearly equal in brightness.]

from celestial space. Two years earlier, Donati showed the light of a small comet to be in part self-emitted, and so not wholly reflected sunshine.

I had myself, in the case of three faint comets, in 1866, in 1867,

The Morehouse Comet. From a Harvard Observatory photograph.

and January 1868, discovered that part of their light was peculiar to them, and that the light of the last one consisted mainly of three bright flutings. Intense, therefore, was the great expectancy with

which I directed the telescope with its attached spectroscope to the much brighter comet which appeared in June, 1868.

The comet's light was resolved into a spectrum of three bright bands or flutings, each alike falling off in brightness on the more refrangible side. On the evening of the 22nd, I measured the positions in the spectrum of the brighter beginnings of the flutings on the red side. I was not a little surprised the next morning to find that the three cometary flutings agreed in position with three similar flutings in the brightest part of the spectrum of carbon. Some time before, I had mapped down the spectrum of carbon, from different sources, chiefly from different hydrocarbons. In some of these spectra, the separate lines, of which the flutings are built up, are individually more distinct than in others. The comet bands, as I had seen them on the previous evening, appeared to be identical in character in this respect, as well as in position in the spectrum, with the flutings as they appeared when I took the spark in a current of olefiant gas. I immediately filled a small holder with this gas, arranged an apparatus in such a manner that the gas could be attached to the end of the telescope, and its spectrum, when a spark was taken in it, seen side by side with that of the comet.

Fortunately the evening was fine; and on account of the exceptional interest of confronting for the first time the spectrum of an earthly gas with that of a comet's light, I invited Dr. Miller to come and make the crucial observation with me. The expectation which I had formed from my measures was fully confirmed. The comet's spectrum when seen together with that from the gas agreed in all respects precisely with it. The comet, though "subtle as Sphinx," had at last yielded up its secret. The principal part of its light was emitted by luminous vapour of carbon.

This result was in harmony with the nature of the gas found occluded in meteorites. Odling had found carbonic oxide as well as hydrogen in his meteorite. Wright, experimenting with another type of meteorite, found that carbon dioxide was chiefly given off. Many meteorites contain a large percentage of hydrocarbons; from one of such sky-stones a little later I observed a spectrum similar to that of the comet. The three bands may be seen in the base of a candle flame.

SECCHI[1]

The First General Spectral Classification of the Stars

(From "On Stellar Spectrometry," *Report* of the British Association for the Advancement of Science, 1868.)

Fraunhofer was the first to analyze with the prism the light of some of the stars. He discovered in them lines analogous to those which he had discovered in the solar spectrum. Donati, an Italian astronomer now at Florence, resumed these researches and extended their field. Several astronomers followed,[2] and amongst them the distinguished Mr. Huggins, to whom we owe a description of the spectra of a great number of stars and the application of the principle of determining the substances contained in a star from the black lines of absorption which we see in its spectrum, as was proposed by Kirchhoff. Mr. Huggins also made the wonderful discovery of the gaseous state of the nebulæ.

The field opened by these discoveries was immense, and even before the date of Mr. Huggins's publications I tried to glean some ears in it. In the first stage of these studies the principal stars only were examined, the imperfection of my instruments not allowing the examination of all the heavenly bodies.

An optical combination which I had the good fortune to discover, enabled me to extend the researches to the whole of the visible

[1] Angelo Secchi (1818–1878), Italian astronomer, was a Jesuit Father and director of the observatory of the Collegio Romano. His work was almost exclusively in spectrum analysis.

[2] A classification of stellar spectra was suggested in 1862 by Lewis Morris Rutherfurd (1816–1892), American astronomer, in the following paragraph (*Am. J. Sci.*, 1863): The star spectra present such varieties that it is difficult to point out any mode of classification. For the present, I divide them into three groups: first, those having many lines and bands and most nearly resembling the sun, *viz.*, Capella, β Geminorum, α Orionis, Aldebaran, γ Leonis, Arcturus, and β Pegasi. These are all reddish or golden stars. The second group, of which Sirius is the type, presents spectra wholly unlike that of the sun, and are white stars. The third group, comprising α Virginis, Rigel, &c., are also white stars, but show no lines; perhaps they contain no mineral substance or are incandescent without flame.]

stars, and even to several telescopic ones, which present perhaps the greatest mysteries of this kind.

This optical combination consists of a single prism of that kind which is used for direct vision, combined with a cylindrical lens. This combination allows us to employ the full light of the stars, not diminished as in common spectroscopes by absorption, or by a slit and the several surfaces and thicknesses through which the light must pass. The image of the star in this system is formed in the focus as a luminous line of white colour if there is no prism; and with the prism the image is decomposed into a series of luminous lines arranged according to their refrangibilities, the interruptions due to the discontinuity of the light appearing as black lines.

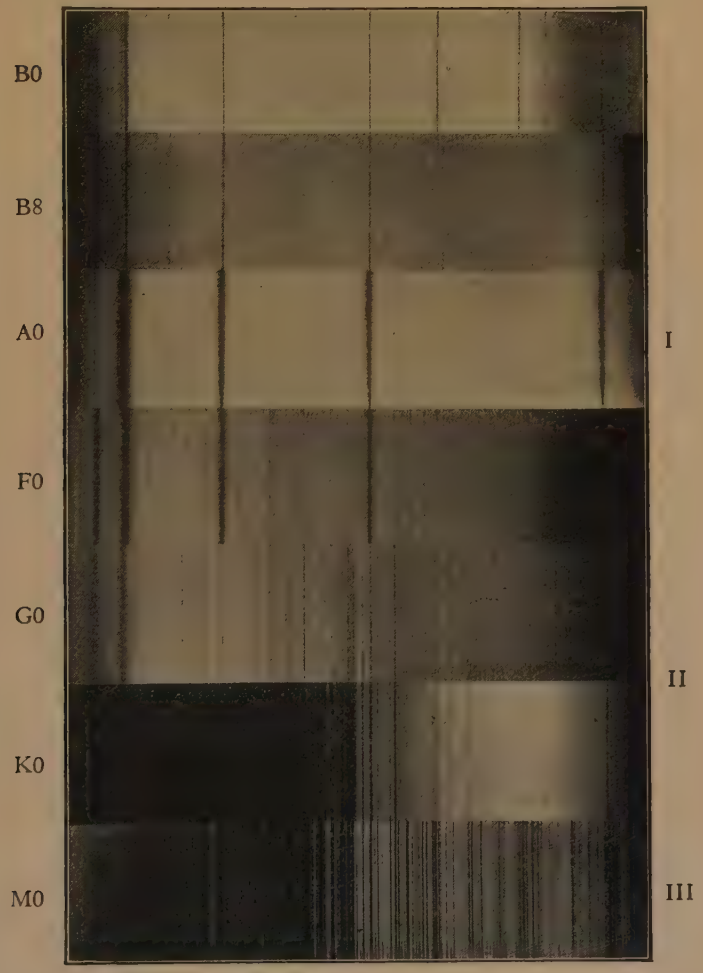
In such a spectrum the relative position of the lines can be measured with a common screw-micrometer; and their absolute position can be determined by comparison with fundamental stars, whose lines, on account of their intensity, can be fixed in an absolute manner relatively to known substances by a common slit-spectroscope. The comparison and measurement are rendered more easy by an improvement introduced in the instrument, by means of which I can see the direct image of the star together with its spectrum. The superposition of this image on a spectral line in a part of the field of the telescope, marked by a wire, is susceptible of great nicety in measurement, and gives very accurate results.

This, in a few words, was the apparatus employed in my researches. This year I have made a considerable improvement by employing an eyepiece made with cylindrical lenses only; with these such an intensity of light is obtained that I have been able to observe the spectra of stars of the seventh and eighth magnitudes, which are of course quite invisible to the naked eye.

Let us come now to the results. Many hundred stars of every magnitude to the sixth were passed in review. A catalogue of the chief of them has been made, and partly published. The work of the last year, yet unpublished, has been especially the examination of the red stars of smaller magnitudes, of which a particular research was instituted, but which was superseded after the reception of the catalogue of Prof. Schjelerup. All the objects contained in this catalogue (printed also in Chamber's "Treatise on Astronomy") have been examined to the eighth magnitude, beyond which limit my instrument cannot give a good spectrum.

The principal results and conclusions at which I have arrived are these:

1st. All the stars in relation to their spectrum can be divided into four groups, for each of which the type of spectrum is quite different.



Stellar spectral types with Draper classification (Harvard) indicated on the left and Secchi's classes on the right. The typical stars are: B0, Epsilon Orionis; B8, Rigel; A0, Sirius; F0, Canopus; G0, Capella; K0, Arcturus; M0, Betelgeuse.

The first type is represented by the stars Sirius, and Vega or α Lyræ, and by all the *white* stars, as α Aquilæ, Regulus, Castor,

the large stars in the Great Bear, α excepted, &c. The spectra of all these stars consist of an almost uniform prismatic series of colours, interrupted only by four very strong black lines. Of these black lines the one in the red is coincident with the solar line *C* of Fraunhofer; another, in the blue, coincides with the line *F*; the other two are also in the sun's spectrum, but they have no prominent place. These lines all belong to hydrogen gas; and the coincidence of these four black lines with those of the gas has been, by careful experiment, already proved by Mr. Huggins, and also lately by myself. In α Lyræ the coincidence is found to be perfectly accurate. Mr. Huggins, however, finds a little difference in the spectrum of Sirius, for which we may account in another way, as I will explain presently.

Stars of this first type are very numerous, and embrace almost one-half of the visible stars of the heavens. We observe, however, some difference in individual stars; so that in some the lines are broader, and in others narrower; this may be due to the thickness of the stratum which has been traversed by the luminous rays. The more vivid stars have other very fine lines occasionally visible, but which are not characteristic of the type-form. In this type the red rays are very faint in proportion to the blue, violet, and green, so that the colour of the star tends to the blue hue, and occasionally to the green. Of this last kind is the group of the large constellation Orion and its neighbourhood.

The second type is that of the yellow stars, as Capella, Pollux, Arcturus, Aldebaran, α Ursæ Majoris, &c. These stars have a spectrum exactly like that of our sun—that is, distinguished by very fine and numerous lines. These stars give occasionally a continuous spectrum, when the state of the atmosphere is not good; but in general the lines may be distinguished very easily. A fuller description is unnecessary, since the spectrum of the sun is very well known. The only thing which deserves particular attention is that in this class occasionally the magnesium lines are very strong, so as to produce very strong bands, and the iron lines in the green are in some very distinct. These stars can be distinguished even without the prism by the difference of colour, a rich yellow, which contrasts strongly with that of the first type. Stars of this second type are very numerous, and embrace almost the other half of the stars.

The third and very remarkable type is that of orange or reddish stars. These have as a prototype the stars α Herculis, α Orionis,

Antares, o Ceti, β Pegasi. The spectra of these stars show a row of columns at least eight in number, which are formed by strong luminous bands alternating with darker ones, so arranged as to represent apparently a series of round pillars, closely resembling a colonnade, α Herculis is exceedingly remarkable in this respect; the other stars are more or less clearly divided into pillars; but it is quite impossible to describe the beauty of the appearance which is visible in a telescope on a fine night.

All the pillars are generally resolved more or less completely in different stars into smaller and finer lines, very sharp and clear. I have carefully drawn, after actual measurements, the spectrum of α Orionis and α Herculis; and in my memoir those of Antares and Aldebaran are given. In these stars some of the divisions of the pillars correspond to some principal lines of Fraunhofer, as *D* and *b*; but others, although very near, do not coincide with them, as *C* and *F*. The presence of hydrogen, however, is certain, the lines *C* and *F* having been found in the principal of them.

The divisions of the pillars after many measurements have been found to agree perfectly in all these stars; so that this type is very constant and well marked. In my catalogue 25 of these most interesting objects are registered; and I do not imagine that I have exhausted the number.

A very interesting feature connects this type with the preceding one. Here I must remark that we have to distinguish between lines and bands of shadow. The lines are strips narrow and sharp, the bands are shaded; although perhaps each band may be composed of very small lines, the aspect with our instruments (as at present constructed) is that of a more or less continuous shade. This shade is analogous to that which is produced by the vapour of our atmosphere in the spectrum of the sun when it is near the horizon.

Now it is a very remarkable fact that these types seem to differ from one another not in the metallic lines, but in the nebulous bands. Thus, for instance, the spectrum of Arcturus and Aldebaran represent the same metallic lines as α Orionis, but this has bands in addition; the feature, however, is altogether so peculiar that a different type must be constituted. It is to be remarked also that all the pillars have their luminous sides toward the red, while the shadowed sides are towards the violet; this difference is very substantial, as we shall see presently . . .

The fourth type is not less remarkable. This is the result of a laborious research on the telescopic stars of a red colour. Some of

these are very small; and none of them exceed the sixth magnitude. This is the reason why in my first memoir I limited the spectra to three types only, being engaged on larger stars only. The spectrum of this type consists of three large bands of light, which alternate with dark spaces so distributed as to have the most luminous side towards the violet.

KIRKWOOD[1]

Gaps in the Asteroid Belt

(From "The Asteroids between Mars and Jupiter," *Annual Report* of the Smithsonian Institution for 1876.)

Distribution of the Asteroids.—As long since as 1857, when the number of known asteroids was less than 50, it was inferred from physical considerations by the author of this paper that great irregularity must obtain in the distribution of these bodies, and that gaps or chasms would be found in those parts of the zone where the periods of asteroids would be commensurable with that of Jupiter. To verify this theory every addition to the group was watched with interest. In 1866, when the number had increased to 88, and the agreement between theory and observation had become quite marked, the attention of astronomers was called to the coincidence by a paper read at the Buffalo meeting of the American Association for the Advancement of Science. The comparison of fact and hypothesis has been continued to the present time; and it is now proposed to show that recent discoveries have confirmed the theory of an irregular distribution . . .

At the distance 3.27—which falls between the orbits of Bertha and Johanna—a planetary mass would make precisely two revolutions while Jupiter completes one. It is obvious, therefore, that all its conjunctions with that planet, for an indefinite period, must occur in the same parts of its path. Consequently its orbit would become more and more eccentric, until, if the nebulous ring had considerable density, the disturbed matter would be brought into contact, either in aphelion or perihelion, with masses having somewhat different velocities. The planetary nucleus formed in this manner would be at some distance from the primitive orbit of the particle disturbed. A gap or chasm would thus be produced at the distance 3.27, or wherever the period of an asteroid would have

[1] Daniel Kirkwood (1815–1895), American astronomer, developed a dynamical theory of the distribution of minor planets, the divisions in Saturn's ring, and the relation between comets and meteors.

to that of Jupiter a simple relation of commensurability. It may be objected that the mutual attractions of those primitive masses were too inconsiderable to result in the formation of planetary nuclei. But even granting the force of this objection, the fact still remains that the orbits of asteroids at the specified distances would increase in eccentricity till the masses in perihelion would reunite with the central nebula. In either case, therefore, chasms would be left in the primitive annulus. Let us now inquire whether the facts presented in Table II[1] sustain the conclusions derived from physical considerations.

The mean distance of Hilda	= 3.9505
That of Flora	= 2.2014
Breadth of zone	= 1.7491

As small bodies in the remoter parts of the ring are more difficult of detection, the zone will be considered under three divisions of equal breadth. The innermost section, *A*, extending from 2.2014 to 2.7844, contains 115 of the 169 asteroids whose elements are known; the mean interval between the consecutive orbits being 0.00511. Now the interval at the distance 2.50—between Thetis and Hestia—where an asteroid's period would be one-third of Jupiter's, is 0.0643, the widest in the section and about twelve times the mean interval. The middle section, *B*, extending from 2.7844 to 3.3675, contains 47 minor planets, the average interval between their consecutive orbits being 0.01268. The widest of these gaps, which is 0.1, or eight times the mean, is found at the distance 3.27, where an asteroid's period would be half that of Jupiter. Finally, the most remote section, *C*, extends from 3.3675 to 3.9505, including seven known asteroids. The mean interval in this section is 0.0972. The widest hiatus (between the orbits of Camilla and Hilda) contains the distances 3.58, 3.70, 3.80, and 3.85. The second in extent (between Sylvia and Camilla) contains the distance 3.51, where nine asteroid periods would be equal to five of Jupiter.

The nine distances at which the periods of minor planets would have simple relations of commensurability to the period of Jupiter are thus found in the widest chasms of the zone. The result may be somewhat modified by future discoveries and by the more exact determination of orbits. The general features of the ring, however,

[1 "A Table of the Elements of the Asteroids."]

depend on too wide an induction of facts to be regarded as merely accidental. The marked irregularity of distribution throughout the cluster is presented at one view in the following table:

	Asteroids
Interior to the distance 2.25	2
From 2.25 to 2.35	7
From 2.35 to 2.45	32
From 2.45 to 2.55	3
From 2.55 to 2.65	26
From 2.65 to 2.75	28
From 2.75 to 2.85	19
From 2.85 to 2.95	6
From 2.95 to 3.05	10
From 3.05 to 3.15	19
From 3.15 to 3.25	9
From 3.25 to 3.35	1
From 3.35 to 3.45	3
From 3.45 to 3.55	2
Exterior to 3.55	2

In three portions of the ring the clustering tendency is here distinctly evident. These are from 2.35 to 2.45, from 2.55 to 2.80, and from 3.05 to 3.20. We have thus an obvious resemblance to the rings of Saturn; the partial breaks or chasms in the asteroid zone corresponding to the well-known intervals in the system of secondary rings. It may be remarked, moreover, that *all but one* of the nineteen asteroids between 2.75 and 2.85 are found in the inner half of the division; the outer half containing the distance at which five periods of a minor planet would be equal to two of Jupiter.

JANSSEN[1]

The First Observation of a Solar Prominence without an Eclipse

(Translated from *Comptes Rendus* de l'Académie des Sciences, Vol. 67, 1868.)

I have just arrived [Cocanada, Sept. 19, 1868] from Guntoor, my station for the observation of the eclipse, and I take advantage of the immediate departure of the courier to give to the Academy some news of the mission with which it did me the honor of entrusting to me.

I lack the time to send a detailed account. I shall have the honor of doing that with the next mail. Today I shall merely summarize the principal results obtained.

The station at Guntoor was without doubt the most favored: the sky was fine, especially during totality, and my powerful telescope of nearly three meters focal length enabled me to pursue the analytical study of all phenomena of the eclipse.

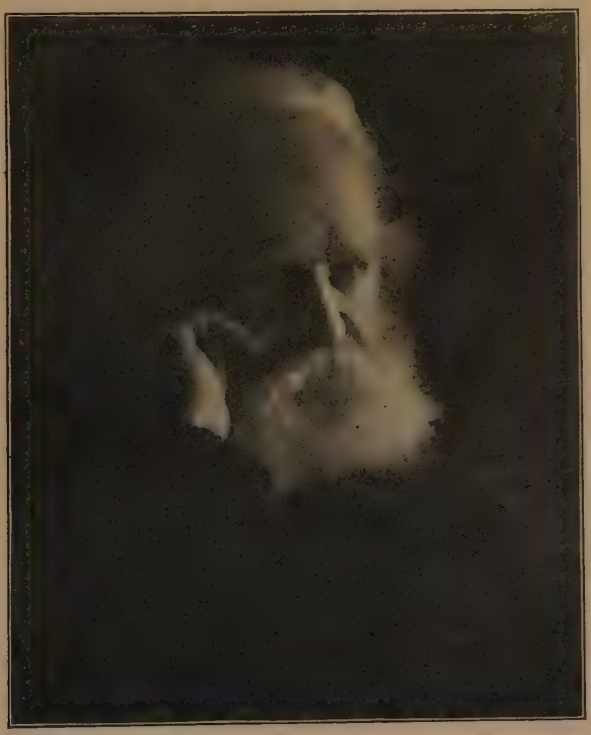
Immediately after totality two magnificent prominences appeared. One of them (more than three minutes in height) blazed with a splendor that is difficult to imagine. The analysis of the light immediately showed me that it was formed by an enormous column of incandescent gas, principally composed of hydrogen.

The analysis of the circumsolar region, where M. Kirchhoff places the solar atmosphere, did not give results conforming with the theory formulated by that illustrious physicist. The results, it seems to me, should lead to a knowledge of the true constitution of the solar spectrum.

But the most important result of these observations is the discovery of a method, the principle of which was conceived during

[1] Pierre Jules César Janssen (1824–1907), French astronomer, was an early investigator of the solar spectrum and became the first director, in 1875, of the Astrophysical Observatory at Meudon. Janssen and Sir Norman Lockyer independently and at about the same time discovered the method of observing solar prominences at the limb of the sun through using a high dispersion spectroscope in order to project the bright hydrogen lines of the prominence spectrum on the much attenuated background of the solar spectrum.

the eclipse itself, and which permits the study of the prominences and of circumsolar regions at all times, without the necessity of having recourse to the interposition of an opaque body before the disk of the Sun. This method is founded on the spectral properties of the light of prominences, light which resolves itself into a small number of highly luminous pencils, corresponding to the dark lines of the solar spectrum.



Janssen.

As early as the day following the eclipse the method was applied with success, and I have thus been able to witness phenomena presented by a new sort of eclipse which lasted during the whole day. The prominences of the preceding day were profoundly modified; there remained scarcely any trace of the great prominence, and the distribution of the gaseous material was entirely different.

Since that day up to the fourth of October I have constantly studied the Sun from this point of view. I have made charts of

the prominences which show with what rapidity (often in a few minutes) these immense gaseous masses change their form and position. Finally, during this period, which has been equivalent to an eclipse of seventeen days, I have gathered a large number of facts, which, just as they stand, give evidence concerning the physical constitution of the Sun.

I am happy to offer these results to the Academy and to the Bureau of Longitudes to justify the confidence which they placed in me and the honor which they conferred in trusting me with this important mission.

HELMHOLTZ[1]

On the Source of the Sun's Heat

(From "On the Origin of the Planetary System," 1871, Popular Scientific Lectures, Vol. 2 (1908 Ed.); translated by E. Atkinson, 1880.)

Of all the subjects to which the thought and imagination of man could turn, the question as to the origin of the world has, since remote antiquity, been the favourite arena of the wildest speculation. Beneficent and malignant deities, giants, Kronos who devours his children, Niflheim, with the ice-giant Ymir, who is killed by the celestial Asas, that out of him the world may be constructed—these are all figures which fill the cosmogonic systems of the more cultivated of the peoples. But the universality of the fact, that each people develops its own cosmogonies, and sometimes in great detail, is an expression of the interest, felt by all, in knowing what is our own origin, what is the ultimate beginning of the things about us. And with the question of the beginning is closely connected that of the end of all things; for that which may be formed, may also pass away. The question about the end of things is perhaps of greater practical interest than that of the beginning . . .

All life and all motion on our earth is, with few exceptions, kept up by a single force, that of the sun's rays, which bring to us light and heat. They warm the air of the hot zones, this becomes lighter and ascends, while the colder air flows from the poles. Thus is formed the great circulation of the trade winds. Local differences of temperature over land and sea, plains and mountains, disturb the uniformity of this great motion, and produce for us the capricious change of winds. Warm aqueous vapours ascend with the warm air, become condensed into clouds, and fall in the

[1] Hermann von Helmholtz (1821–1894), German physicist, first proposed in 1854 that the sun's heat is supplied by gravitational attraction. The current estimates of the requisite amount of energy is, however, at least a hundred times as great as that allowed by the contraction theory, and have led to considerations of atomic transformations as the important source of stellar energy.

cooler zones, and upon the snowy tops of the mountains, as rain and as snow. The water collects in brooks, in rivers, moistens the plains, and makes life possible; crumbles the stones, carries their fragments along, and thus works at the geological transformation of the earth's surface. It is only under the influence of the sun's rays that the variegated covering of plants of the earth grows; and while they grow, they accumulate in their structure organic matter, which partly serves the whole animal kingdom as food, and serves man more particularly as fuel. Coal and lignites, the sources of power of our steam engines, are remains of primitive plants—the ancient production of the sun's rays.

Need we wonder if, to our forefathers of the Aryan race in India and Persia, the sun appeared as the fittest symbol of the Deity? They were right in regarding it as the giver of all life—as the ultimate source of almost all that has happened on earth.

But whence does the sun acquire this force? It radiates forth a more intense light than can be attained with any terrestrial means. It yields as much heat as if 1500 pounds of coal were burned every hour upon each square foot of its surface. Of the heat which thus issues from it, the small fraction which enters our atmosphere furnishes a great mechanical force. Every steam-engine teaches us that heat can produce such force. The sun, in fact, drives on earth a kind of steam-engine whose performances are far greater than those of artificially constructed machines. The circulation of water in the atmosphere raises, as has been said, the water evaporated from the warm tropical seas to the mountain heights; it is, as it were, a water-raising engine of the most magnificent kind, with whose power no artificial machine can be even distantly compared. I have previously explained the mechanical equivalent of heat. Calculated by that standard, the work which the sun produces by its radiation is equal to the constant exertion of 7000 horse-power for each square foot of the sun's surface . . .

Let us return to the special question which concerns us here: Whence does the sun derive this enormous store of force which it sends out?

On earth the processes of combustion are the most abundant source of heat. Does the sun's heat originate in a process of this kind? To this question we can reply with a complete and decided negative, for we now know that the sun contains the terrestrial elements with which we are acquainted. Let us select from among

them the two, which, for the smallest mass, produce the greatest amount of heat when they combine; let us assume that the sun consists of hydrogen and oxygen, mixed in the proportion in which they would unite to form water. The mass of the sun is known, and also the quantity of heat produced by the union of known weights of oxygen and hydrogen. Calculation shows that under the above supposition, the heat resulting from their combustion would be sufficient to keep up the radiation of heat from the sun for 3021 years. That, it is true, is a long time, but even profane history teaches that the sun has lighted and warmed us for 3000 years, and geology puts it beyond doubt that this period must be extended to millions of years.

Known chemical forces are thus so completely inadequate, even on the most favourable assumption, to explain the production of heat which takes place in the sun, that we must quite drop this hypothesis.

We must seek for forces of far greater magnitude, and these we can only find in cosmical attraction. We have already seen that the comparatively small masses of shooting-stars and meteorites can produce extraordinarily large amounts of heat when their cosmical velocities are arrested by our atmosphere. Now the force which has produced these great velocities is gravitation. We know of this force as one acting on the surface of our planet when it appears as terrestrial gravity. We know that a weight *raised from the earth* can drive our clocks, and that in like manner the gravity of the water rushing down from the mountains works our mills.

If a weight falls from a height and strikes the ground its mass loses, indeed, the visible motion which it had as a whole—in fact, however, this motion is not lost; it is transferred to the smallest elementary particles of the mass, and this invisible vibration of the molecules is the motion of heat. Visible motion is transformed by impact, into the motion of heat.

That which holds in this respect for gravity, holds also for gravitation. A heavy mass, of whatever kind, which is suspended in space separated from another heavy mass, represents a force capable of work. For both masses attract each other, and, if unrestrained by centrifugal force, they move towards each other under the influence of this attraction; this takes place with ever-increasing velocity; and if this velocity is finally destroyed, whether this be suddenly, by collision, or gradually, by the friction of movable parts, it develops the corresponding quantity of

the motion of heat, the amount of which can be calculated from the equivalence, previously established, between heat and mechanical work.

Now we may assume with great probability that very many more meteors fall upon the sun than upon the earth, and with greater velocity, too, and therefore give more heat. Yet the hypothesis, that the entire amount of the sun's heat which is continually lost by radiation, is made up by the fall of meteors, a hypothesis which was propounded by Mayer,[1] and has been favourably adopted by several other physicists, is open, according to Sir. W. Thomson's investigations, to objection; for, assuming it to hold, the mass of the sun should increase so rapidly that the consequences would have shown themselves in the accelerated motion of the planets. The entire loss of heat from the sun cannot at all events be produced in this way; at the most a portion, which, however, may not be inconsiderable.

If, now, there is no present manifestation of force sufficient to cover the expenditure of the sun's heat, the sun must originally have had a store of heat which it gradually gives out. But whence this store? We know that the cosmical forces alone could have produced it. And here the hypothesis, previously discussed as to the origin of the sun, comes to our aid. If the mass of the sun had been once diffused in cosmical space, and had then been condensed —that is, had fallen together under the influence of celestial gravity—if then the resultant motion had been destroyed by friction and impact, with the production of heat, the new world produced by such condensation must have acquired a store of heat not only of considerable, but even of colossal, magnitude.

Calculation shows that, assuming the thermal capacity of the sun to be the same as that of water, the temperature might be raised to 28,000,000 degrees, if this quantity of heat could ever have been present in the sun at one time. This cannot be assumed, for such an increase of temperature would offer the greatest hindrance to condensation. It is probable rather that a great part of this heat, which was produced by condensation, began to radiate into space before this condensation was complete. But the heat which the sun could have previously developed by its condensation, would have been sufficient to cover its present expenditure for not less than 22,000,000 of years of the past.

[1 Robert Mayer (1814–1878) in 1848 made the first attempt to found an interpretation of the Sun's source of heat on energy principles.]

And the sun is by no means so dense as it may become. Spectrum analysis demonstrates the presence of large masses of iron and of other known constituents of the rocks. The pressure which endeavours to condense the interior is about 800 times as great as that in the centre of the earth; and yet the density of the sun, owing probably to its enormous temperature, is less than a quarter of the mean density of the earth.

We may, therefore, assume with great probability that the sun will still continue in its condensation,[1] even if it only attained the density of the earth—though it will probably become far denser in the interior owing to the enormous pressure—this would develop fresh quantities of heat, which would be sufficient to maintain for an additional 17,000,000 of years the same intensity of sunshine as that which is now the source of all terrestrial life.

[1 The contraction theory proposed by Helmholtz was later developed by J. Homer Lane (1869), by August Ritter (1878), by Lord Kelvin (1887), and by others.]

AN EXTRACT FROM THE WILL OF JAMES LICK

(Recorded in the Office of the Recorder of the County of San Francisco, State of California, In Liber 810, of Deeds, pp. 26, *et seq.;* Nov. 10, 1875.)

In trust however, for the uses and purposes hereinafter mentioned, and to have and to hold the same unto the said parties of the second part, their successors and assigns, in trust for the following purposes, to-wit: . . .

The Lick Observatory, Mount Hamilton, California.

Third—To expend the sum of seven hundred thousand dollars ($700,000) for the purpose of purchasing land, and constructing and putting up on such land, as shall be designated by the party of the first part, a powerful telescope, superior to and more powerful than any telescope everyet made, with all the machinery appertaining thereto and appropriately connected therewith, or that is necessary and convenient to the most powerful telescope now in use, or suited to one more powerful than any yet con-

structed; and also a suitable observatory connected therewith. The parties of the second part hereto, and their successors shall, as soon as said telescope and observatory are constructed, convey the land whereupon the same may be situated, and the telescope, observatory, and all the machinery and apparatus connected therewith, to the corporation known as the "Regents of the University of California"; and, if after the construction of said telescope and observatory, there shall remain of said seven hundred thousand dollars in gold coin any surplus, then said parties of the second part shall turn over such surplus to said corporation, to be invested by it in bonds of the United States, or of the City and County of San Francisco, or other good and safe interest-bearing bonds, and the income thereof shall be devoted to the maintenance of said telescope and the observatory connected therewith, and shall be made useful in promoting science; and the said telescope and observatory are to be known as "The Lick Astronomical Department of the University of California."

YOUNG[1]

The Corona Line and the Flash Spectrum

(From *The American Journal of Science and Arts*, 1871.)

Our spectroscopic results completely confirm those of last year, except that the two faint lines, which I saw between *D* and *E* last year and suspected to be corona lines as well as 1474, were not seen at all this time; 1474 was traced by Prof. Winlock to a distance of nearly 20′ from the sun's limb.[2] I traced it 16′ on the West, 12′ on the North, 14′ on the East, and about 10′ on the South. The principal chromosphere lines were also visible in the corona to a distance of 3′ or 4′. Prof. Winlock and myself both agree in attributing this to the reflection of the haze around the sun. I am more confident as to this, because last year, in a clear atmosphere, the *C* line was certainly sharply terminated at the upper limit of the chromosphere or prominences under observation.

But the most interesting spectroscopic observation of the eclipse appears to me to be the ascertaining at the base of the chromosphere, and, of course, in immediate contact with the photosphere, of a thin layer in whose spectrum the dark lines of the ordinary solar spectrum are all reversed. Just previous to totality, I had carefully adjusted the slit tangential to the sun's limb at the point where the second contact would take place, and was watching the gradual brightening of 1474 and the magnesium lines. As the crescent grew narrower, I noticed a fading out, so to speak, of all the dark lines in the field of view, but was not at all prepared for the beautiful phenomenon which presented itself when the moon finally covered the whole photosphere. Then the whole field was at once filled with brilliant lines, which suddenly flashed into brightness and then gradually faded away until, in less than two

[1] Charles Augustus Young (1834–1908), American astronomer. His important contributions to knowledge of the nature of the sun include spectroscopic researches on the corona, the reversing layer, sunspots, and prominences.

[2] The line 1474 corresponds in present nomenclature to 5317 Ångstroms; the correct value of the wave-length is now known to be 5303Å.]

seconds, nothing remained but the lines I had been watching. The slit was very close, and the definition perfect.

Of course, I cannot positively assert that all the bright lines held exactly the same position that had been occupied by dark ones previously, but I feel very sure of it, as I particularly noticed several groups, and the whole arrangement and relative intensity struck me as perfectly familiar.

This observation is a confirmation of Secchi's continuous spectrum at the edge of the sun, and I think tends to make tenable the original theory of Kirchhoff as to the constitution of the sun and the origin of the dark lines in the ordinary solar spectrum.

ASAPH HALL[1]

Discovery of the Satellites of Mars

(From "The Satellites of Mars," 1878.)

In the spring of 1877, the approaching favorable opposition of the planet Mars attracted my attention, and the idea occurred to me of making a careful search with our large Clark refractor for a satellite of this planet. An examination of the literature of the planet showed, however, such a mass of observations of various kinds, made by the most experienced and skillful astronomers, that the chance of finding a satellite appeared to be very slight, so that I might have abandoned the search had it not been for the encouragement of my wife. A more complete examination of the observations also gave some encouragement, as it showed that hardly any astronomer since the time of Sir William Herschel had made a special search for satellites. It is evident from his notes that Herschel was searching for satellites in 1783, and his failure to find any seems to have convinced astronomers that none existed; and the statement that "Mars has no moon" became current in our textbooks. The only astronomer of recent times whose doubt of the prevailing opinion was strong enough to induce him to make a thorough search for a satellite was Professor D'Arrest, formerly Director of the Observatory at Copenhagen. A reference to D'Arrest's search is made by Dr. Klein in his "Handbook of Astronomy," Vol. 1, p. 140; and a more complete statement is given by D'Arrest himself in the *Astronomische Nachrichten*, Vol. 64, p. 74. I inferred that D'Arrest made his search during the favorable opposition of Mars in 1862, but I am not certain that this was really the case, and perhaps D'Arrest missed the favorable opportunity, and did not make his search until 1864. D'Arrest died in June, 1875; but through the kindness of Professor Schjellerup, the present Director of the Observatory at Copenhagen, I learn that D'Arrest's handbook shows that he made an earnest search for satellites, but

[1] Asaph Hall (1829–1907), American astronomer, with the 26-inch refractor at Washington found the rotation period of Saturn and discovered the satellites of Mars.

failed to find any. In his statement in the *Astronomische Nachrichten*, D'Arrest assumes a distance of Mars from the earth equal to 0.52, and with an assumed value of the mass of the planet he computes the apparent elongation of a satellite that would revolve around the planet in a given number of days. He shows that a satellite at an elongation of 70′ would have a period greater than the period of Mars around the sun, or greater than 687 days, and hence infers that it is useless to search beyond the distance of 70′. The fact that D'Arrest, who was a skillful astronomer, had searched in vain was discouraging; but remembering the power and excellence of our glass, there seemed to be a little hope left. The southern declination of the planet in the opposition of 1877 was, however, against us, and the chances seemed to be in favor of the powerful reflector at Melbourne.

My search for a satellite was begun early in August, as soon as the geocentric motion of the planet made the detection of a satellite easy. At first, my attention was directed to faint objects at some distance from the planet; but all these proving to be fixed stars, on August 10, I began to examine the region close to the planet, and within the glare of light that surrounded it. This was done by sliding the eye-piece so as to keep the planet just outside the field of view, and then turning the eye-piece in order to pass completely around the planet. On this night I found nothing. The image of the planet was very blazing and unsteady, and the satellites being at that time near the planet, I did not see them. The sweep around the planet was repeated several times on the night of the 11th, and at half past two o'clock I found a faint object on the following side and a little north of the planet, which afterward proved to be the outer satellite. I had hardly time to secure an observation of its position when fog from the Potomac River stopped the work. Cloudy weather intervened for several days. On the night of August 15, the sky cleared up at eleven o'clock and the search was resumed; but the atmosphere was in a very bad condition, and nothing was seen of the object, which we now know was at that time so near the planet as to be invisible. On August 16, the object was found again on the following side of the planet, and the observations of that night showed that it was moving with the planet, and, if a satellite, was near one of its elongations. On August 17, while waiting and watching for the outer satellite, I discovered the inner one. The observations of the 17th and 18th put beyond doubt the character of these objects,

and the discovery was publicly announced by Admiral Rodgers. Still, for several days the inner moon was a puzzle. It would appear on different sides of the planet in the same night, and at

The 26 inch telescope of the U. S. Naval Observatory, with which Asaph Hall discovered the two satellites of Mars.

first I thought there were two or three inner moons, since it seemed to me at that time very improbable that a satellite should revolve around its primary in less time than that in which the primary

rotates. To decide this point I watched this moon throughout the nights of August 20 and 21, and saw that there was in fact but one inner moon, which made its revolution around the primary in less than one-third the time of the primary's rotation, a case unique in our solar system.

Of the various names that have been proposed for these satellites, I have chosen those suggested by Mr. Madan of Eton, England, viz:

DEIMOS for the outer satellite;
PHOBOS for the inner satellite.

These are generally the names of the horses that drew the chariot of Mars; but in the lines referred to they are personified by Homer, and mean the attendants, or sons of Mars. These lines occur in the Fifteenth Book of the Iliad, where Ares is preparing to descend to the earth to avenge the death of his son. Bryant's translation is as follows:

> He spake, and summoned Fear and Flight to yoke
> His steeds, and put his glorious armor on.

GOULD[1]

The Belt of Bright Stars

(From *The American Journal of Science and Arts*, 1874.)

It has generally been assumed that the number of visible stars of any given magnitude—whether brighter or fainter—diminishes as their distance from the Milky Way increases. In the elevated position and pure atmosphere of Cordoba, this nebulous circle is seen with a vividness far surpassing that to which we are accustomed here, and, moreover, most of that portion which lies in the southern hemisphere is intrinsically brighter than the northern half; so that its position is far more clearly defined than I have ever seen it elsewhere. And few celestial phenomena are more palpable there than the existence of a stream or belt of bright stars, including *Canopus*, *Sirius*, and *Aldebaran*, together with the most brilliant ones in *Carina*, *Puppis*, *Columba*, *Canis Major*, *Orion*, &c., and skirting the Milky Way on its preceding side. When the opposite half of the Galaxy came into view, it was almost equally manifest that the same is true there also, the bright stars likewise fringing it on the preceding side, and forming a stream which, diverging from the Milky Way at the stars *Alpha* and *Beta Centauri*, comprises the constellation *Lupus* and a great part of *Scorpio*, and extends onward through *Ophiuchus* toward *Lyra*. Thus a great circle or zone of bright stars seems to gird the sky, intersecting with the Milky Way at the Southern Cross, and manifest at all seasons, although far more conspicuous upon the Orion side than on the other. Upon my return to the North, I sought immediately for the northern place of intersection; and although the phenomenon is by far less clearly perceptible in this hemisphere, I found no difficulty in recognizing the node in the constellation *Cassiopea*, which is diametrically opposite to *Crux*. Indeed it is easy to fix the right ascension of the northern node at about

[1] Benjamin Apthorp Gould (1824–1896), American astronomer, is known for his great catalogues of southern stars, the founding of the *Astronomical Journal*, and the first determination of longitude difference between Europe and America by means of the transatlantic cable.

0^h 50^m, and that of the southern one at 12^h 50^m; the declination being in each case about 60°, so that these nodes are very close to the points at which the Milky Way approaches most nearly to the poles. The inclination of this stream to the Milky Way is about 25°, the Pleiades occupying a position midway between the nodes.[1]

A considerable portion of the bright stars of our firmament is situated within this zone or stream, or in its immediate vicinity. It has been a source of surprise to me that it had not previously attracted the notice of astronomers, and since writing the foregoing paragraphs I had begun the preparation of some data in statistical form to demonstrate its existence, when I discovered that it had been alluded to by Sir J. Herschel in his Results of Observations at the Cape of Good Hope. His words are as follows (p. 385):

"It is . . . in the interval between η *Argus* and α *Crucis* that the Galactic circle, or medial line of the Milky Way, may be considered as crossed by that of the zone of large stars which is marked out by the brilliant constellation of *Orion*, the bright stars of *Canis Major* and almost all the more conspicuous stars of *Argo*, the *Cross*, the *Centaur*, *Lupus*, and *Scorpio*. A great circle passing through ϵ *Orionis* and α *Crucis* will mark out the axis of the zone in question, whose inclination to the galactic circle is therefore about 20°, and whose appearance would lead us to suspect that our nearest neighbors in the sidereal system (if really such) form part of a subordinate sheet or stratum, deviating to that extent from the general mass which seen projected on the heavens forms the Milky Way."

Yet he does not appear to have recognized the fact that this zone of bright stars may be traced with tolerable distinctness through the entire circuit of the heavens, forming a great circle as well defined as that of the galaxy itself.

[1] Apparently a slip in locating the Pleiades, which are but a quarter of the angular distance from the northern node.]

NEWCOMB[1]

Researches on the Motion of the Moon

(From Appendix II, "Washington Astronomical and Meteorological Observations," Vol. 22, 1878.)

For several years after the publication of Hansen's Tables of the Moon, it was very generally believed that the theory of the motion of that body, after having been the subject of astronomical and mathematical research for two thousand years, was at last complete, and that, in consequence, the motion of the moon could now be predicted with the same accuracy as that of the other heavenly bodies. In 1870, the writer showed that this belief was entirely unfounded, and that the correctness of the tables since 1750 had been secured only by sacrificing the agreement with observations previous to that epoch, so that, about 1700, Hansen's Tables deviated more widely from observations than did those which they superseded. It was also shown that at the time of writing, the moon was falling behind the tabular position at a rate which would speedily cause a very serious error in the representation of the Tables. Altogether, it appeared that notwithstanding the immense improvement which *Hansen* had made in the accuracy of the inequalities of short period, the theory of those of long period was no nearer such a solution as would agree with observation than when it was left by *Laplace* . . .

In all theories of the moon before the beginning of the last century, the mean motion of that body was supposed to be uni-

[1] Simon Newcomb (1835–1909), American astronomer, superintendent of the Nautical Almanac, made some of the most important contributions of his century to knowledge of the fundamental constants of astronomy. His studies led to significant results concerning motions of the four inner planets and of the moon, the orbit of Saturn's satellite Hyperion, and the distribution of the stars. He wrote also in the field of popular science, biography, economics, and fiction. His discussion (quoted here) of discordances between observation and theory for the inner planets, has been important in modern hypotheses of gravitation and relativity.

form. The first inequality discovered was the secular acceleration. While the general proposition that a comparison of ancient and modern eclipses shows the mean motion of the moon to have increased since the time of *Ptolemy* is no doubt due to *Halley*, I believe the first careful determination of its amount is that by *Dunthorne* . . .

From the year 1750 to the present time, we have a nearly continuous series of occultations and eclipses, observed with a high degree of accuracy at observatories whose positions are well known, notably those of Greenwich and Paris. Of course, these observations become more and more numerous as we approach the present time. Let us next inquire how accurately the mean motion of the moon can be determined from these observations. I conceive that between the epochs 1780 and 1820 we shall find at least 150 well-observed occultations. If we omit a third of these as being cases where the star was too far from the line of motion of the moon's center to give a good determination of the moon's longitude, we shall have 100 left suitable for this determination. If we take the probable error of each longitude derived from a single occultation as 2″.0, which I think is not far from the truth, the probable error of the mean of all will be 0″.20, and the epoch will be about 1800. Allowing for systematic differences of observers, it may be increased to 0″.30. Again, from the more numerous observations on both sides of the epoch 1875, we may hope to obtain the moon's mean longitude for that epoch with the same precision. By a comparison of the two, the moon's mean motion during the first seventy years of the present century will be obtained, with a probable error of 0″.4, the corresponding epoch being 1837. The two epochs compared, conjoined with the observations between 1820 and 1850, will give the mean longitude for 1837 with a probable error which, for our present purposes, may be regarded as insignificant.

Now, suppose that, with the mean longitude and mean motion thus determined, we carry back the position of the moon to the epochs of the observations previous to 1720, and, considering the difference as due solely to the secular acceleration, determine the latter from the comparison of the observed and computed longitudes, what will be the probable errors of the several results? The probable error of the computed mean longitude will be, with sufficient approximation $0''.6T$; T being the number of centuries from 1837. If we represent by e the probable error of the mean

longitude derived from observation, the probable error of the comparison will be

$$\sqrt{0''.36T^2 + e^2},$$

and the probable error of the value of the secular acceleration deduced from the comparison will be

$$\frac{\sqrt{0''.36T^2 + e^2}}{T^2} = \epsilon$$

The values of the several quantities which we have estimated for each series of observations of other data are given in the following table, the last series of numbers being the probable error of the secular acceleration which would result from a comparison of the observations with a lunar theory derived from observations between 1780 and 1875.

Data or observers	T	e	ϵ
Ptolemy's eclipses	21.4	200″	0″.4
Arabian eclipses	8.8	60	0.8
Bullialdus and *Gassendus*	1.95	5	1.3
Hevelius	1.6	3	1.0
Paris and Greenwich astronomers	1.35	0.6	0.6

From this reasoning, we may draw the following conclusion. *Granting the fundamental premises on which we have reasoned, the secular acceleration of the moon can be determined with nearly the same order of accuracy from the modern as from the ancient observations.*

The writer is quite conscious that the degree of accuracy here assigned to the results is something to be hoped for rather than expected, and that many astronomers may consider, not without some reason, that the degree of precision attainable has been greatly exaggerated. To judge of the precise state of the question, it may not be amiss to present some considerations on the premises, expressed or implied, from which we have reasoned. They are substantially:

1. That a theory can be constructed which shall accurately represent the real and apparent inequalities of long period in the

moon's mean longitude. This involves the three conditions that the motion of the moon is affected only by the gravitation of the known bodies of the solar system; that the effect of this gravitation can be accurately calculated; and that the motion of rotation of the crust of the earth upon its axis is invariable, a uniform secular retardation excepted. A failure in any one of these conditions will destroy the basis of the preceding calculation, and will increase the probable error of the results derivable from the modern observations much more than in the case of the ancient ones. It is useless to speculate upon the probability that these conditions will be fulfilled.

2. The other hypothesis is that the observations are not affected by any systematic error nearly as great as the probable error of the mean derived from each series of observations. Among the sources of such systematic errors are to be included erroneous longitudes of observatories, constant instrumental errors in the determination of time, and any habit peculiar to the observer by which he systematically observes an occultation differently from the transit of the sun's limb or of a star.

I do not think that these errors will very largely increase the probable error of the results, because occultations, if actually observed, are peculiarly free from systematic error. If we take the probable errors which we have supposed for the moon's longitude at the three epochs 1700, 1800, and 1870, and reduce them to time, they will amount to about $1^s.2$, $0^s.6$, and $0^s.6$ respectively, quantities far greater than the average observed personal equations between different observers. Now, we have tacitly supposed it an even chance that the mean of a series of observations extending over a period of forty years, made by a number of different observers, and in a number of different ways, did not exceed 1^s during the interval 1680–1720, and $0^s.5$ during the interval 1780–1820, and I do not think this estimate will sound extravagant. In regard to the possible erroneous difference of longitude between Paris and Greenwich, it is to be remarked that observations were made at both these places during the intervals we have been considering, and in such numbers that the error will be nearly eliminated from the lunar elements.

However, a certain and perhaps very sensible increase in the probable errors of the results is no doubt to be looked for; but a partial or entire set-off against them is to be found in the fact that more observations are actually available than we have supposed to

be included in forming the basis of our theory, and, when these are added, the precision of the result will be sensibly increased.

In the preceding enumeration, I have included only classes or series of observations. In addition to these, there is a great number of isolated observations, both ancient and modern, of every variety of excellence, which I have not deemed it necessary to enumerate, because their value can be determined only by comparison and discussion. They will, of course, add slightly to the accuracy of the data for the final determination of the required element. Altogether, I think there would be room to hope that we might obtain the secular acceleration from the modern observations alone, with the probable error of scarcely more than half a second, if only the long-period inequalities in the moon's motion were conclusively settled. This is something which is still in the future . . .

Discordances in the Secular Variations of the Inner Planets

(From "Fundamental Constants of Astronomy," 1895.)

The present investigations are founded on a comparison of the secular variations derived purely from observations, with those resulting from the values of the masses obtained independently of the secular variations. For the sake of clearness, these two sets of secular variations and their differences are collected in the following table.[1] The mean errors assigned to the theoretical values are those which result from the probable mean errors of the respective masses. They are therefore not to be regarded as independent. The mean errors given in the column of differences are those which result from a combination of those of the other two columns. The errors of the observed quantities must not, however, be judged from those of the differences, because subsequent changes in the masses of Mercury, Venus, and the Earth may produce a general diminution in the discordances.

[1 For each planet values are given for the secular variations in eccentricity, longitude of perihelion, inclination, and longitude of node.]

	Observation	Theory	Difference
		MERCURY	
$D_t e$	+ 3″.36 ± 0″.50	+ 4″.24 ± 0″.01	− 0″.88 ± 0″.50
$eD_t\pi$	+ 118.24 ± 0.40	+109.76 ± 0.16	+ 8.48 ± 0.43
$D_t i$	+ 7.14 ± 0.80	+ 6.76 ± 0.01	+ 0.38 ± 0.80
$\sin i\, D_t\theta$	− 91.89 ± 0.45	− 92.50 ± 0.16	+ 0.61 ± 0.52
		VENUS	
$D_t e$	− 9.46 ± 0.20	− 9.67 ± 0.24	+ 0.21 ± 0.31
$eD_t\pi$	+ 0.29 ± 0.20	+ 0.34 ± 0.15	− 0.05 ± 0.25
$D_t i$	+ 3.87 ± 0.30	+ 3.49 ± 0.14	+ 0.38 ± 0.33
$\sin i\, D_t\theta$	− 105.40 ± 0.12	− 106.00 ± 0.12	+ 0.60 ± 0.17
		EARTH	
$D_t e$	− 8.55 ± 0.09	− 8.57 ± 0.04	+ 0.02 ± 0.10
$eD_t\pi$	+ 19.48 ± 0.12	+ 19.38 ± 0.05	+ 0.10 ± 0.13
$D_t \epsilon$	− 47.11 ± 0.23	− 46.89 ± 0.09	− 0.22 ± 0.27
		MARS	
$D_t e$	+ 19.00 ± 0.27	+ 18.71 ± 0.01	+ 0.29 ± 0.27
$eD_t\pi$	+ 149.55 ± 0.35	+ 148.80 ± 0.04	+ 0.75 ± 0.35
$D_t i$	− 2.26 ± 0.20	− 2.25 ± 0.04	− 0.01 ± 0.20
$\sin i\, D_t\theta$	− 72.60 ± 0.20	− 72.63 ± 0.09	+ 0.03 ± 0.22

If we multiply the mean errors given by 0.6475, to reduce them to probable errors, we shall see that only four of the fifteen differences are less than their probable errors. The deviations which call for especial consideration are the following four:

1. The motion of the perihelion of Mercury. The discordance in the secular motion of this element is well known.

2. The motion of the node of Venus. Here the discordance is more than five times its probable error.

3. The perihelion of Mars. Here the discordance is three times its probable error.

4. The eccentricity of Mercury. The discordance is more than twice its probable error. It is to be remarked, however, that the probable error of this quantity is very largely a matter of judgment, and that its value may have been underestimated.

The deviations, if not due to erroneous masses, may be explained on two hypotheses. One is that propounded by Prof. *Hall*, that the gravitation of the Sun is not exactly as the inverse square, but that the exponent of the distance is a fraction greater than 2 by a certain minute constant. This hypothesis accounts for only the motions of the perihelia, and not for any other discordances.

The other hypothesis is that of the action of unknown masses or arrangements of matter. Since the latter hypothesis would account for other motions than those of the perihelia, it might seem that the existence of the other discordances tells very strongly in its favor. The hypotheses of possible distributions of unknown matter, therefore, have first to be considered.

Hypothesis of Non-sphericity of the Sun.—In a case where our ignorance is complete, all hypotheses which do not violate known facts are admissible. Beginning at the center and passing outward, the first question arises whether the action may not be due to a non-spherical distribution of matter within the body of the Sun, resulting in an excess of its polar over its equatorial moment of inertia. The theory of the Sun which has in recent times been most generally accepted is that its interior may be regarded as gaseous, or rather as a form of matter which combines the elasticity and mobility of a gas with the density of a liquid. Such being the case, we may conceive that vortices of which the axes coincide with that of rotation may exist in the interior in such a way that the surfaces of equal density are non-spherical. A very small inequality of this sort would suffice to account for the motion of the perihelion of Mercury.

This hypothesis admits of an easy test. Whatever be the nature or amount of the inequality, a simple computation shows that to account for the observed phenomenon it is necessary and sufficient that the equipotential surfaces at the surface of the Sun should have an ellipticity of rather more than half a second of arc. It can not, I conceive, be doubted that the visible photosphere is an equipotential surface. We have then to inquire whether there is any such ellipticity of the photosphere as that required by the hypothesis. This question seems completely set at rest by the great mass of heliometer measures made by the German observers in connection with the transits of Venus of 1874 and 1882, which have been discussed by Dr. *Auwers*. The general result is that the mean of the equatorial measures are slightly less than the mean of the polar measures, the differences, however, being within the probable errors of the results. I conclude that there can be no such non-symmetrical distribution of matter in the interior of the Sun as would produce the observed effect.

This same conclusion seems to apply to matter immediately around the photosphere. An equatorial ring of planetoids, or gaseous substances of the required mass, very near the photosphere,

would render the equipotential surfaces of the photosphere elliptical to a degree which seems precluded by the measures in question. At a very short distance from the surface, however, the effect would be inappreciable.

Hypothesis of an Intra-mercurial Ring or Group of Planetoids. —Passing outward, we have next to consider the hypothesis of an intra-mercurial ring adequate to produce the observed phenomena. In a first approximation we may suppose the ring circular. Its mass can not be determined, because it will depend upon the distance; we have to determine a certain function of the mass, and distance adequate to produce the observed motion of the perihelion. Then we must inquire what effect the ring will have on the secular variations of the other elements, both of Mercury and of the other planets, and see if these effects can be reconciled with observation. In the computations, I have assigned to the excess of motion the provisional value 40″.7. If the ring is not very distant from the Sun the motion which it will produce in the perihelion of a planet whose mean motion is n and whose mean distance is a may be represented in the form

$$D_t\pi = \frac{\mu n}{a^2}$$

μ being a function of the mass of the ring and of its radius, which is nearly the same for all of the planets, so long as the radius of the ring is only a small fraction of the distance of Mercury . . .

The great inclination seems in the highest degree improbable if not mechanically impossible, since there would be a tendency for the planes of the orbits of a ring of planets so situated to scatter themselves around a plane somewhere between that of the orbit of Mercury and that of the invariable plane of the planetary system, which is nearly the same as that of the orbit of Jupiter. Moreover, the motion of the perihelion of Mars is still unaccounted for and that of the node of Venus only partially accounted for, as shown by the large residual of the second equation. In fact, the great inclination assigned to the ring comes from the necessity of representing as far as possible the latter motion.

There would of course be no dynamical impossibility in the hypothesis of a single planet having as great an inclination as that required. But I conceive that a planet of the adequate mass could not have remained so long undiscovered. Whether we regard the matter as a planet or a ring, a simple computation shows that its mass, if at the Sun's surface, would be about $\frac{1}{1650}$ that of the

Sun itself, and one-fourth of this if at a distance equal to the Sun's radius. We may conceive, if we can not compute, how much light such a mass of matter would reflect, Altogether, it seems to me that the hypothesis is untenable.

Hypothesis of an Extended Mass of Diffused Matter Like That Which Reflects the Zodiacal Light.—The phenomenon of the zodiacal light seems to show that our Sun is surrounded by a lens of diffused matter which extends out to, or a little beyond, the orbit of the Earth, the density of which diminishes very rapidly as we recede from the Sun. The question arises whether the total mass of this matter may not be sufficient to cause the observed motion.

So far as the action of that portion of matter which is near the Sun is concerned, the conclusions just reached respecting a ring surrounding the Sun will apply unchanged, because we may regard such a mass as made up of rings. Observation seems to show that the lens in question is not much inclined to the ecliptic, and if so it would produce a motion of the nodes of Venus and Mercury the opposite of that indicated by the observations.

There is another serious difficulty in the way of the hypothesis. A direct motion of the perihelion of a planet may be taken as indicating the fact that the increase of its gravitation toward the Sun as it passes from aphelion to perihelion is slightly greater than that given by the law of the inverse square. This increase would be produced by a ring of matter either wholly without or wholly within the orbit. But if we suppose that the orbit actually lies in the matter composing such a ring, the effect is the opposite; gravitation toward the Sun is relatively diminished as the planet passes from aphelion to perihelion, and the motion of the perihelion would be retrograde.

It can not be supposed that that part of the zodiacal light more distant from the Sun than the aphelion of Mercury is even as dense as that portion contained between the aphelion and the perihelion distances. The result in question must, therefore, be due wholly to that part of the matter which lies near to the Sun, and we thus have all the difficulties of the intra-mercurial ring theory, with one more added.

Hypothesis of a Ring of Planetoids between the Orbits of Mercury and Venus.—It appears that any ring or zone of matter adequate to produce the observed effect must lie between the orbits of Mercury and Venus. Its assignment to this position requires a more careful determination of its possible eccentricity. There will

be six independent elements to be determined; the mass, the mean distance, the eccentricity, the perihelion, the inclination, and the node.

I find that the observed excesses of motion of the elements of Mercury and Venus will be approximately represented by elements not differing much from the following:

Total mass of group	$\frac{1}{37{,}000{,}000}$
Mean distance	0.48
Eccentricity of orbit	0.04
Longitude of perihelion	10°
Longitude of node	35°
Inclination to ecliptic	7°.5
Probable diameter at distance unity if agglomerated into a single planet	3″.5

Considerations on the Admissibility of the Hypothesis—Possible Mass of the Minor Planets.—Although the preceding hypothesis is that which best represents the observations of Mercury and Venus, we can not, in the present condition of knowledge, regard it as more than a curiosity. True, it is plausible at first sight. Since, as already remarked, any disturbing body of sufficient mass to cause the observed excess of motion of the perihelion of Mercury would change the position of the planes of the orbits, and since observations give apparent indications of such a change in the plane of the orbit of Venus, it might appear that we have here a very good ground for the view that all the motions are due to the attraction of unknown masses. But the great difficulty is that the excess of motion of the orbital planes is in the opposite direction from what we should expect. A group of bodies revolving near the plane of the ecliptic would produce a retrograde motion of the nodes. But the observed excess is direct. A direct motion can be produced only in case the orbits are more inclined than those of the disturbed planet. In admitting such orbits we encounter difficulties which, if not absolutely insurmountable, yet tell against the probability of the hypothesis.

The hypothesis carries with it the probable result that the excess of motion of the perihelion of Mars is produced by the action of the minor planets. I have considered the question of this action in an unpublished investigation. From the probable albedo and magnitude of the minor planets and the observations of *Barnard* and others on their diameters, I have determined the probable mass of each part of the group having a given opposition magni-

tude. The result is that the number of these bodies having such a magnitude appears to progress in a fairly uniform manner through several magnitudes. The ratio of progression may lie anywhere between the limits 2 and 3. Up to the limit 3 the total mass, if continued on to infinity, could not produce any appreciable effect on the motion of Mars. But if we suppose a larger ratio than 3 to prevail, then the number of planets of smaller magnitude would be so numerous as to form a zone of light across the heavens, as may readily be seen by considering that the total amount of light reflected from the planets of each order of magnitude would form an increasing series, since the ratio between the brilliancies of two objects of unit difference in magnitude is only about 2.5. We may, therefore, suppose that the faint band of light which is said to be visible across the entire heavens as a continuation of the zodiacal light, as well as the "gegenschein," is due to these minute bodies, and yet find their total mass too small to produce any appreciable effect.

Whether we can assign to the components of such a group any magnitude so small that they would be individually invisible, and a number so small that they would not be seen collectively as a band of light brighter than the zodiacal arch, and yet having a total mass so large as to produce the observed effects, is a very important question which can not be decided without exact photometric investigations. It is, however, certain that if we could do so we should have to suppose a very unlikely discontinuity in the law of progression between each magnitude and the number of bodies having that magnitude. It must, therefore, suffice for our present object that we regard the hypothesis of such bodies as unsatisfactory.

Hypothesis That Gravitation toward the Sun Is Not Exactly as the Inverse Square of the Distance.—Professor *Hall's* hypothesis seems to me provisionally not inadmissible. It is, that in the expression for the gravitation between two bodies of masses m and m' at the distance r

$$\text{Force} = \frac{mm'}{r^n}$$

the exponent n of r is not exactly 2, but $2 + \delta$, δ being a very small fraction. This hypothesis seems to me much more simple and unobjectionable than those which suppose the force to be a more or less complicated function of the relative velocity of the bodies. On this hypothesis the perihelion of each planet will have a direct

motion found by multiplying its mean motion by one-half the excess of the exponent of gravitation.

Putting

$$n = 2.000\,000\,1574$$

the excess of motion of each perihelion of the four inner planets would be as follows. It will be seen that the evidence in the case of Venus and the Earth is negative, owing to the very small eccentricities of their orbits, while the observed motion in the case of Mars is very closely represented.

	$D_{t\pi}$	$eD_{t\pi}$
Mercury	42″.34	8″.70
Venus	16.58	0.11
Earth	10.20	0.17
Mars	5.42	0.51

An independent test of this hypothesis in the case of other bodies is very desirable. The only case in which there is any hope of determining such an excess is that of the Moon, where the excess would amount to about 140″ per century. This is very nearly the hundred thousandth part of the total motion of the perigee. The theoretical motion has not yet been computed with quite this degree of precision. The only determination which aims at it is that made by *Hansen*. He finds

	Theory	Observation	Difference
Annual motion of perigee	146,434″.04	146,435″.60	+1″.56
Annual motion of node	−69,676.76	−69,679.62	−2.86

The observed excess of motion agrees well with the hypothesis, but loses all sustaining force from the disagreement in the case of the node. The differences *Hansen* attributes (wrongly, I think) to the deviation of the figure of the Moon from mechanical sphericity.

Consistency of Hall's Hypothesis with the General Results of the Law of Gravitation.—The law of the inverse square is proven to a high degree of approximation through a wide range of distances. The close agreement between the observed parallax of the Moon and that derived from the force of gravitation on the Earth's surface shows that between two distances, one the radius of the Earth and the other the distance of the Moon, the deviation from the law of the square can be only a small fraction of the thousandth part, or, we may say, a quantity of the order of magnitude of the five-thousandth part.

Coming down to smaller distances, we find that the close agreement between the density of the Earth as derived from the attraction of small masses, at distances of a fraction of a meter, with the density which we might *a priori* suppose the Earth to have, shows that within a range of distance extending from less than one meter to more than six million meters, the accumulated deviation from the law can scarcely amount to its third part. The coincidence of the disturbing force of the Sun upon the Moon with that computed upon the theory of gravitation, extends the coincidence from the distance of the Moon to that of the Sun, while *Kepler's* third law extends it to the outer planets of the system. Here, however, the result of observations so far made is relatively less precise. We may therefore say, with entire confidence, as a result of accurate measurement, that the law of the inverse square holds true within its five-thousandth part from a distance equal to the Earth's radius to the distance of the Sun, a range of twenty-four thousand times; that it holds true within a third of its whole amount through the range of six million times from one meter to the Earth's radius; and within a small but not yet well-defined quantity from the distance of the Sun to that of Uranus, in which the multiplication is twentyfold.

If *Hall's* hypothesis contradicted these conclusions it would be untenable. But a very simple computation will show that, assuming the force to vary as $r^{-(2+\delta)}$, δ being a minute constant sufficient to account for the motion of the perihelion of Mercury, the effect would be entirely inappreciable in the ratio of the gravitation of any two bodies at the widest range of distance to which observation has yet extended. Although the total action of a material point on a spherical surface surrounding it would converge to zero when the radius became infinite, instead of remaining constant, as in the case of the inverse square, yet the diminution in the action upon a surface no larger than would suffice to include the visible universe would be very small.

The Abnormal Behavior of the Perihelion of Mercury

(From "Astronomical Papers of the American Ephemeris," Vol. 1, 1882.)

The series of transits of Mercury discussed by *Leverrier* terminated with that of 1848. The work of *Leverrier* on this subject is memorable from the fact that in it was first pointed out the dis-

crepancy between the motion of the perihelion of Mercury as derived from observation and as derived from theory. The existence of this discrepancy, at least when the mass of Venus determined in other ways is employed, has been placed beyond doubt by observations of four transits since the publication of *Leverrier's* work. Notwithstanding the thoroughness with which the great astronomer treated the subject, there are a number of circumstances which render a reinvestigation desirable. The mere fact that a third of a century of observations, far more accurate than any made in previous times, are now available, is alone a reason for ascertaining whether any modification of *Leverrier's* results is now possible.

Again, several new questions have arisen which observed transits of Mercury will help to decide. Among these is the question of the uniformity of the earth's rotation. The discrepancies between theory and observation in the moon's secular acceleration, and the inequalities of long period in its longitude, give rise to the question whether the rotation of the earth itself may not be variable. That a slow secular retardation in this rotation exists seems almost certain on theoretical grounds; and it is not impossible that causes may act, capable of producing changes of long period. The question whether the apparent discrepancies in the moon's motion are to be accounted for in this way can best be settled by observation on other rapidly moving bodies.

There are many questions respecting the phenomena of contact, data for the solution of which will be found in the mass of recorded observations of transits of Mercury. Valuable hints may thus be derived in respect to the interpretation and discussion of observed transits of Venus.

Yet another reason for a new discussion is the desirableness of having the results of the theory and of observation so worked out, collated, and compared that the astronomer of the present or future may be able to see how they are to be reconciled without the necessity of going over the entire discussion from the beginning. The author hopes that the present paper will make this possible . . .

Numerical Comparison of Observed and Tabular Quantities, with the Resulting Equations of Condition.—In the first of the following tables is given the comparison of the observed and tabular times of contacts, to be subsequently used as a check upon the equations of condition. The following columns are the ones which seem to need explanation.

COMPARISON OF OBSERVED AND TABULAR GEOCENTRIC CONTACTS

I. November Transits, Interior Contacts

Contact	Date	Observed G.M.T. of geocentric contact	Weight	Tabular times, with symbolic corrections
		h m s s		h m s
II	1677, Nov. 6	21 34 1 − 33k	0.1	21 33 31.6 + 1.1N + 4.8V + 6.3M − 10.7R' + 15.6R
III	1677, Nov. 7	2 47 27 − 33k	0.1	2 48 12.9 − 1.4 + 4.7 − 6.4 + 10.7 − 15.6
III	1697, Nov. 2	19 42 53 − 26k	0.3	19 42 47.4 + 4.2 + 4.8 + 1.7 + 13.9 − 20.3
II	1723, Nov. 9	2 26 52 − 17k	2.0	2 26 55.0 + 1.7 + 4.8 + 5.5 − 11.1 + 16.2
II	1736, Nov. 10	21 10 30 − 9k	1.0	21 10 28.3 + 8.5 + 5.0 − 2.2 − 21.4 + 31.3
III	1736, Nov. 10	23 48 51 − 9k	1.0	23 49 1.3 − 8.5 + 4.5 + 2.3 + 21.0 − 30.7
II	1743, Nov. 4	20 14 21 − 4k	1.0	20 14 24.2 − 3.4 + 4.7 − 7.0 − 12.5 + 18.3
III	1743, Nov. 5	0 45 5 − 4k	1.5	0 45 6.4 + 3.1 + 4.8 + 7.0 + 12.5 − 18.2
II	1769, Nov. 9	7 22 47 + 12k	1.0	7 22 42.5 + 2.4 + 4.8 + 6.5 − 11.7 + 17.2
III	1769, Nov. 9	12 9 51 + 12k	0.2	12 9 54.2 − 2.6 + 4.7 − 6.6 + 11.7 − 17.2
III	1782, Nov. 12	2 42 6 + 17k	3.0	2 41 59.5 + 22.1 + 5.3 − 23.1 − 49.5 + 72.5
III	1782, Nov. 12	3 49 37 + 17k	3.0	3 50 9.5 − 21.8 + 4.2 + 22.6 + 48.4 − 70.9
II	1789, Nov. 5	0 53 2 + 18k	2.0	0 53 7.4 − 2.5 + 4.7 − 6.3 − 11.6 + 17.0
III	1789, Nov. 5	5 44 12 + 18k	1.0	5 44 16.9 + 2.2 + 4.8 + 6.2 + 11.5 − 16.9
III	1802, Nov. 8	23 41 5 + 16k	3.0	23 41 8.4 − 0.4 + 4.7 − 1.1 + 10.3 − 15.1
II	1822, Nov. 4	13 3 42 + 9k	0.5	13 3 45.6 − 8.5 + 4.5 − 11.8 − 21.0 + 30.7
III	1822, Nov. 4	15 45 18 + 9k	1.0	15 45 13.6 + 8.3 + 4.9 + 11.9 + 21.0 − 30.7
II	1848, Nov. 8	23 6 47 − 0k	5.0	23 6 42.8 + 0.7 + 4.8 + 6.6 − 10.5 + 15.3
III	1848, Nov. 9	4 28 8 − 0k	0.3	4 28 4.1 − 0.9 + 4.7 − 6.6 + 10.5 − 15.3
II	1861, Nov. 11	17 20 16 − 2k	0.7	17 20 17.9 + 4.3 + 4.8 − 8.8 − 14.2 + 20.7
III	1861, Nov. 11	21 18 20 − 2k	5.0	21 18 16.8 − 4.5 + 4.6 + 8.8 + 14.1 − 20.6

II	1868, Nov. 4	17 27 0	$-10k$	0.5	17 27 43.6	− 5.7	+ 4.6	− 4.5	− 15.9	+ 23.3
III	1868, Nov. 4	21 0 9.8	$-10k$	6.0	20 59 57.5	+ 5.4	+ 4.9	+ 4.4	+ 15.9	− 23.3
II	1881, Nov. 7	10 18 38	$-16k$	3.0	10 18 15.5	− 1.3	+ 4.7	+ 6.1	− 10.6	+ 15.6
III	1881, Nov. 7	15 35 54	$-16k$	3.0	15 35 37.3	+ 1.1	+ 4.8	− 6.1	+ 10.6	− 15.6

II. May Transits, Interior Contacts

II	1740, May 2	9 43 9	$-6k$	0.1	9 41 50.8	+ 34.2N	+ 10.4V	+ 13.8M	− 43.6R'	− 78.9R
III	1753, May 5	22 6 0.5	$+2k$	1.5	22 5 52.4	+ 2.7	+ 12.9	− 9.7	+ 16.1	− 29.1
II	1786, May 3	14 59 25 15 0 8	$+18k$	0.3	15 0 26.6	+ 14.0	+ 11.7	− 9.4	− 22.2	+ 40.2
III	1786, May 3	20 21 27	$+18k$	2.0	20 21 0.5	− 12.8	+ 13.4	+ 9.6	+ 22.6	− 40.9
II	1799, May 6	21 9 42	$+17k$	1.5	21 9 44.9	− 4.0	+ 12.7	− 17.1	− 16.3	+ 29.5
III	1799, May 7	4 30 32	$+17k$	2.0	4 30 37.8	+ 5.5	+ 12.1	+ 16.9	+ 16.3	− 29.5
II	1832, May 4	21 3 30	$+6k$	3.0	21 3 46.7	+ 8.2	+ 12.0	+ 17.4	− 17.7	+ 32.0
III	1832, May 5	3 46 40	$+6k$	3.0	3 46 55.2	− 6.7	+ 12.9	− 17.4	+ 17.7	− 32.0
II	1845, May 8	4 23 50	$+2k$	4.0	4 24 14.6	− 8.0	+ 12.9	− 6.9	− 18.5	+ 33.5
III	1845, May 8	10 49 7	$+2k$	4.0	10 49 34.1	+ 9.5	+ 11.8	+ 7.0	+ 18.5	− 33.5
II	1878, May 6	3 15 49.2	$-15k$	6.0	3 16 6.6	+ 4.6	+ 12.1	− 10.2	− 15.8	+ 28.7
III	1878, May 6	10 43 41.2	$-15k$	4.0	10 44 0.5	− 3.2	+ 12.8	+ 10.3	+ 16.1	− 29.1

The third column gives the Greenwich mean times of geocentric contact derived from the observations already given in Part I. Since, however, the time itself, as determined from astronomical observations, hypothetically needs a correction $-K\Delta t$, this correction is added symbolically to the observed time to render it strictly comparable with the tabular time.

The next column gives the adopted weights of the observation, which refers, not merely to the time, but to the distance of centers. No precise formula has been applied in determining these weights because of the extremely heterogenous character of the data. As a rule, the result of five fairly accordant and satisfactory observations of internal contact is considered entitled to weight 1. But the weight is not proportional to the number of observations, but varies in a less degree, so that 6 is the maximum weight for any one transit. Moreover, account is taken of the skill of the observers and the general accordance and certainty of the observations.

Next we have the tabular times, the computation of which has already been given, followed by the symbolic corrections produced by corrections to the elements.

The observed secular variation of the perihelion of Mercury, as derived from observation, can, without difficulty, be accounted for by suitably increasing the adopted mass of Venus. The only argument against such an increase is that the variations of other elements will not then be represented. But in the absence of any reason for preferring one determination to another, the true form in which we should put the result is that the variations of different elements give different values of the mass of Venus. We can reject one result only when we have found that all the methods but one give accordant results and that this one alone is discordant. The first step toward a satisfactory solution of the question is, therefore, to find what values of the mass of Venus are given by different data and discuss the discordances among them.

Five methods are available for the determination of the mass of Venus.

I. *The Secular Motion of the Perihelion of Mercury.* More exactly we should say the secular motions of V and W, which arise from variations both in the eccentricities and in the perihelion of Mercury and the earth.

II. *The Secular Motion of the Node of Mercury.* Any uncertainty that may exist in the theoretical motion of this node arises

almost entirely from the uncertainty in the mass of Venus, since the influence of all the other planets can be accurately determined.

III. *The Secular Motion of the Node of Venus on the Ecliptic.* Properly speaking we should say the secular motion of the ecliptic itself, because that portion of the motion of the node of Venus which depends on the mass of that planet arises solely from the motion of the ecliptic.

IV. *The Secular Diminution of the Obliquity of the Ecliptic.* This, like the last, is a motion of the ecliptic due to the action of Venus. Hence these two determinations cannot be considered as wholly independent, though each would strengthen the other.

V. *The Periodic Perturbations of Mercury and the Earth Produced by the Action of Venus.* Since a discordance of the kind in question indicates the continuous action of some unknown cause, we cannot say that any one of the first four methods is necessarily free from the effects of such action. Hence, if the results are discordant, we have no right to deduce with certainty any mass of Venus from them. It is different with the last method. It is beyond all moral probability that any unknown cause should produce periodic inequalities in the planetary motions corresponding to those produced by the action of the planets on each other. We may, therefore, consider the mass of Venus derived from periodic perturbations to be that which is to be accepted as the real mass to be used in comparing the other results. Unfortunately, the best mass that can be derived from transits is very uncertain, while that of discussing the meridian observations will be very laborious . . .

We have now the following results for the mass of Venus:

From perihelion of Mercury $1 \div 1000m'$	$= 347.8$
From node of Mercury	408.4
From periodic inequalities	396

while the results from the other two sources will probably not differ much from the second of the above values.

The third value is too uncertain to permit of any conclusion being drawn from its deviation from the second. By merely supposing the constant k to have the value 0.295 not only will the last value be increased to 401, but the value 408.4 from the motion of the node will be diminished. The two values will, therefore, be made more accordant.

There is, therefore, a decided preponderance of evidence that the true value of the mass of Venus does not differ much from

$\frac{1}{405,000}$, and is probably contained between the limits $\frac{1}{400,000}$ and $\frac{1}{410,000}$. The value $\frac{1}{347,800}$ is entirely inconsistent with all the others. We must, therefore, conclude that *the discordance between the observed and theoretical motions of the perihelion of Mercury, first pointed out by Leverrier, really exists, and is indeed larger than he supposed.*

Determination of Excess of Motion of Perihelion.—In investigating the actual amount of the discordance we call to mind that we have no certain evidence as to how the discordance is to be divided among the several elements which enter into the expressions for V' and W'. But, so far as has yet been noticed, it does not appear that any other element than the perihelion of Mercury is affected by this abnormal variation. We, therefore, put the inquiry into this form: assuming that the variations of e, e', and π' correspond to theory, how much is the variation of π in excess of the value given by theory? In considering this question we shall assume $v' = -0.008$ and hence $m' = \frac{1}{405,000}$. We shall also put,

p, the excess in the centennial motion of π . . .

Hence, the excesses of observation over theory, which is to be reduced to zero by attributing suitable values to P and δn, are

$$\begin{aligned} 20''.48 &- 0.487p + 1.487\delta n \\ 12.41 &- 0.284p - 0.716\delta n \end{aligned}$$

Equating these expressions to zero, we find

$$\begin{aligned} \delta n &= +\ 0''.37 \\ p &= +42.95 \end{aligned}$$

It follows that the observed centennial motion of the perihelion of Mercury is greater by 43″ than the theoretical motion computed from the best attainable values of the masses of the planets.

LANGLEY[1]

The Quantity and Quality of the Sun's Radiation

(From "Researches on Solar Heat," 1883, *Professional Papers* of the Signal Service, No. 15, 1884.)

It is the present belief that we know with something like accuracy the amount of heat which the sun sends the earth, and also that we know in general how the atmosphere acts in keeping the earth warm by letting the solar rays pass into it and keeping back others from the soil. It has been usual to state that the extreme violet rays are not readily transmitted by our atmosphere; that of the light rays about one-fifth are absorbed and four-fifths are transmitted, while that as we go on through the extreme red to the dark heat rays we find the absorption growing greater and greater, the dark heat rays being found in very small quantity because they are absorbed almost wholly. It is commonly added that it is owing to this same cause that the heat, which freely enters as light, escapes with difficulty when returned as dark heat in the longer rays corresponding to the lowest portion of the solar heat spectrum, and that thus the atmosphere acts to the earth a part somewhat like that of the glass cover of a hot-bed, materially aiding the solar radiation in maintaining the temperature of the planet. The ordinary conception of this heat-storing action then involves the conclusion that the dark heat of the known solar spectrum is less transmissible than the light heat, and this presumed necessity may possibly have in some degree biased physicists in their investigations on this region, where experiments are difficult. However this may be, they have been on this point provided with little evidence, so that it was rather taken for granted as a supposed necessity, than indubitably demonstrated by sufficient experiment, that the dark heat region of the solar

[1] Samuel Pierpont Langley (1834–1906), American scientist, secretary of the Smithsonian Institution from 1887 to 1906, is known for his pioneer work in aviation, as well as for his fundamental researches on solar radiation, his development of physical apparatus for the study of the sun, and his establishment of the Astrophysical Observatory of the Smithsonian Institution.

spectrum was comparatively non-transmissible by the terrestrial atmosphere. A comfirmatory circumstance to this belief was the fact that Tyndall, and after him others, had proved, by actual experiment, that, to such kinds of heat as come from terrestrial sources of very low temperatures, vapors and gases known to be important constituents of our atmosphere were almost impermeable. Such investigations, it will be remembered, had been made almost solely by the prism, and there was no way known of learning what the wave lengths of this dark heat really were, physicists depending for their knowledge of these wave lengths on certain formulæ which had never been verified, as they did also in a much more important matter, the determination of the amount of heat which the earth's atmosphere diverted from the direct radiation of the sun; a determination which was often made in a way which seemed to assume that nature had spared us all the trouble possible, by here conducting the whole train of her ordinarily mysterious operations, in a way so simple that the formula expressing them was itself as elementary as we could possibly wish.

To know what kind of heat was radiated from the soil we should need to know the wave lengths of heat of this quality, of which even now we remain in ignorance. Draper, in 1881, gives the limit of the solar heat spectrum at a wave length of about 0.001 of a millimeter. M. Becquerel, in a memoir in the *Annales de Chimie et de Physique*, so late as August, 1883, places the ultimate limit of the known spectrum at less than 0.0015 millimeters, and most explicitly approves the statement that these heat rays are less transmissible by the atmosphere than others. Our own measures, here given, add the very remarkable absorption band Ω with others, and extend the directly observed spectrum to a wave length of nearly 0.003 millimeters, while making it probable that the transmissibility of the atmosphere *increases* up to nearly this point, where it suddenly ceases, as if all beyond were an unlimited cold body.

Having been led by the study of selective absorption to think that the portion of the sun's radiation reflected by particles of dust or mist, or otherwise dispersed in our atmosphere, is far larger than is commonly supposed, and that the little-known processes by which it is thus withheld are of importance in their bearing on problems of the widest interest, we commenced in 1880, at the Allegheny Observatory, the study of the solar heat by an instrument (the bolometer) specially invented with the object of doing

for this heat what the eye in the visible prismatic spectrum does for light, that is, of discriminating between one heat ray and another, and we have been able to use it so as to determine, together with the hitherto unknown wave length of a great number of dark-heat rays, the hitherto unknown amount of heat actually observed in each of these near the sea level, and to tell approximately the hitherto equally unknown amount of heat in each of these dark rays before it was absorbed by our atmosphere. The results of these investigations went to show that the heat in most of the known dark-heat wave lengths, instead of being absorbed by this atmosphere, was most freely transmitted by it, a conclusion directly opposed to the common belief, and, if true, of importance, for all the few known observations of physicists seemed to prove the contrary, and meteorologists had generally accepted these supposed observations. Continuing the heat measurements in the "light" region, I found that the heat existed there indeed in greater quantity than in the "dark-heat" region, and yet that it had been already greatly more absorbed, so that the original quantity of heat here must have been enormous as compared with that in the dark-heat region . . .

The earth's actual mean surface temperature being about 15° or 16° (Centigrade), and it being admitted that the heat from the interior, from the stars, from the dynamic effect of meteorites, and in general from all other sources, is negligible, it will follow that if we know the laws under which this heat enters and escapes from our atmosphere, we can determine what amount must be supplied to the earth from without to maintain this known annual temperature of 15 or 16 degrees. The time has not yet probably come for doing this with certainty, but this method is so wholly independent of the others that it may be interesting to us to know its results. Pouillet's data in this respect have been modified somewhat by recent observation. Accepting them, however, as approximately true, we must admit, if we follow his ingenious course of reasoning (but reject his hypothesis of an enormous heat radiation from the stars), that the solar radiation is represented by 3.13 calories. We have fixed already, from the nature of the observations on Mount Whitney, as an upper limit to the solar constant the value 3.5 calories, and as a lower limit the value 2.6 calories. Between these limits we have three independent determinations, without including the method of Pouillet. I have already given the reasons which make me deem it unadvisable to attempt to

assign weights to these determinations and to combine them by any conventional rule.

The reader has had before him in the preceding pages a detailed statement of the observations and methods which have led to these results, and my own inference from them is that in the present condition of our knowledge it is impossible to fix any value of the solar constant with the precision which used to be assigned to it ere the difficult conditions of the actual problem were known, though I think that it has been clearly shown in the preceding pages that this solar constant is greater than has ordinarily been believed.

My conclusion is that in view of the large limits of error we can adopt *three calories as the most probable value of the solar constant*, by which I mean that at the earth's mean distance, in the absence of its absorbing atmosphere, the solar rays would raise one gramme of water three degrees Centigrade per minute for each normally exposed square centimeter of its surface.[1]

This is approximately 126,550,000 ergs per square centimeter per minute. Expressed in terms of melting ice, it implies a solar radiation capable of melting an ice-shell 54.45 meters deep annually over the whole surface of the earth. Somewhat less than two-thirds of this amount reaches us at the sea level ordinarily from a zenith sun, but unless very great precautions are exercised we are apt to undervalue this directly received amount. It follows, then, that the selective absorption of our atmosphere is not only more diverse in kind, but that the total atmospheric absorption is far greater in amount than has been commonly supposed.

On other important points our conclusions are as follows: (1) That although the actual solar radiation is thus largely increased, yet the temperature of the earth's surface is not due principally to this direct radiation, but to the quality of selective absorption in our atmosphere, *without which the temperature of the soil in the tropics under a vertical sun would probably not rise above* −200° C. Nearly all the 215 or more degrees of difference between this and the actual mean temperature of the planet's surface is due to this selective absorption, which accumulates the heat, though in a manner which has not been hitherto correctly understood. It should be understood that these researches have here a practical bearing of great consequence. The temperature of this planet,

[1 Langley's successors in the Astrophysical Observatory have shown in recent years that the Solar Constant is a little less than two calories.]

and with it the existence, not only of the human race, but of all organized life on the globe, appears, in the light of the conclusions reached by the Mount Whitney expedition, to depend far less on the direct solar heat than on the hitherto too little regarded quality of *selective* absorption in our atmosphere, which we are now studying. (2) Generally speaking, the radiation which we see enter we see escape within the utmost limits of the known solar spectrum. The heat-storing action, from checked re-radiation, to which the surface temperature of this planet is due, apparently goes on *beyond* these limits where no spectral measurements have yet been made. *No such wave lengths as those belonging to the heat radiated from the soil, we believe, have ever entered our atmosphere from the sun*, though we admit their existence in the solar spectrum before absorption.

KAPTEYN AND GILL[1]

Concerning the Cataloguing of Southern Stars

(From Cape Observatory *Annals*, Vol. 3, 1896.)

Professor J. C. Kapteyn to Sir David Gill:

Leiden, December 16th, 1885.

. . . I must here break off because this letter has to be dispatched an hour earlier than I expected. I will, therefore, write you another letter that will reach you a week later. In that letter I will make bold to explain to you a proposal that I hope you will not consider indelicate. It is, in the main, what follows: If you will confide to me one or two of the negatives I will try my hand at them, and, if the result proves as I expect, I would gladly devote some years of my life to this work, which would disburden you a little, as I hope, and by which I would gain the honour of associating my name with one of the grandest undertakings of our time.

Kapteyn to Gill:

Leiden, December 23rd, 1885.

I have still to explain to you the proposal in my former letter, which I thought it better not to postpone—my resolution being taken. In doing this, you will excuse me in premising so much about my private circumstances as seems necessary for the purpose.

In the year 1878 I was appointed Professor of Astronomy and Theoretical Mechanics at the University of Groningen, having been before, during a couple of years, Observer at the Leiden Observatory. Directly on my appointment I proposed to the

[1] Jacob Cornelius Kapteyn (1851–1922), Dutch astronomer, professor in the University of Groningen, is best known for the discovery and study of the Two Star Streams, the organization and work on the Kapteyn Selected Areas, and for observational and theoretical work on the distribution of stars and the structure of the universe. His remarkable devotion to science is indicated in the present correspondence, which preceded the undertaking of the Cape Photographic Durchmusterung of Southern Stars.

Sir David Gill (1843–1914), British astronomer, director of the Royal Observatory at the Cape of Good Hope, is known especially for his work on the parallaxes of the sun and the brighter stars.

Government to fit out a little Observatory where, besides instruments for teaching purposes, a Heliometer of 6 inches aperture would be mounted . . .

Perhaps I shall succeed after some years in getting one or two instruments with which truly scientific research may be prosecuted; but at all events a very long time will have to elapse before any such result may be looked for.

The first years of my Professorship once passed, my lectures left me considerable leisure, which it has been always my desire to devote to astronomical observations . . .

Now, after your success in Stellar photography, and especially after your letter in which you tell me "I am obliged to crave help where I can get it," it has occurred to me that by measuring and reducing your photographs I could contribute very effectually towards the success of an enormous and eminently useful undertaking. Since then I have revolved the idea in my mind and I have come to the conclusion that if you will let me, and if I can secure the necessary help, there is no one can be in better conditions to undertake this work than myself.

The former point being granted, it is my plan as to the latter—

1st. To request the Government to double the yearly subsidy of somewhat more than £40 that is granted for the acquisition of books and small astronomical instruments.

2nd. To request another subsidy of £80 for a series of consecutive years from the Society of Teyler, a very rich society, that is always very willing to bestow some money on really scientific pursuits.

With this sum I think I can procure the constant help of three persons to do the most mechanical part of the work, the copying and the simple arithmetical processes, while I myself would execute all the measurements, the computation of the tables of reduction, the comparison of catalogues, &c., for which work perhaps now and then the help of a student could be secured . . .

Having once got the necessary information and tried the necessary experiments, a rough estimate of the time to be expended on the work may be made, and I will then be able to make you a definite proposal, stating the approximate time in which, and the approximate accuracy with which, I could undertake the whole business. Supposing that you are willing to leave the thing to other hands at all, I do not doubt but that we will soon agree as to these points . . .

I have kept this letter here some days to talk the matter of the Photographic Durchmusterung over with Professor Bakhuyzen and his brother. I am bound to say that they were not very enthusiastic about the matter; of course they thought the results, once reached, of immense value, but the drudgery to be gone through before these results are once got into the form of a catalogue almost unbearable. However, I think my enthusiasm for the matter will be equal to (say) six or seven years of such work.

Gill to Kapteyn:

Cape of Good Hope, January 9, 1886.

Such a letter as yours of December 16th requires an immediate answer; I refer, of course, to its concluding portion, in which you offer some years of your life to co-operation with me in cataloguing the Photographic Durchmusterung of the Southern Heavens . . .

Naturally, before you commit yourself to so serious a work, you desire to see a sample of the photographs on which so much labour is to be expended, accordingly I send you two photographs representing the same area . . . [Here follows a long account of instrumental details.]

Gill to Kapteyn:

Cape of Good Hope, January 22nd, 1886.

It will, I hope, be as satisfactory to you as it has been to me, that we have mutually, and almost simultaneously, confided to each other the objects of our work, our hopes, and our difficulties. I with too much on hand, you with too little—both interested in precisely the same kind of work, and both intent on having such work done.

NORMAN LOCKYER[1]

Stellar Evolution

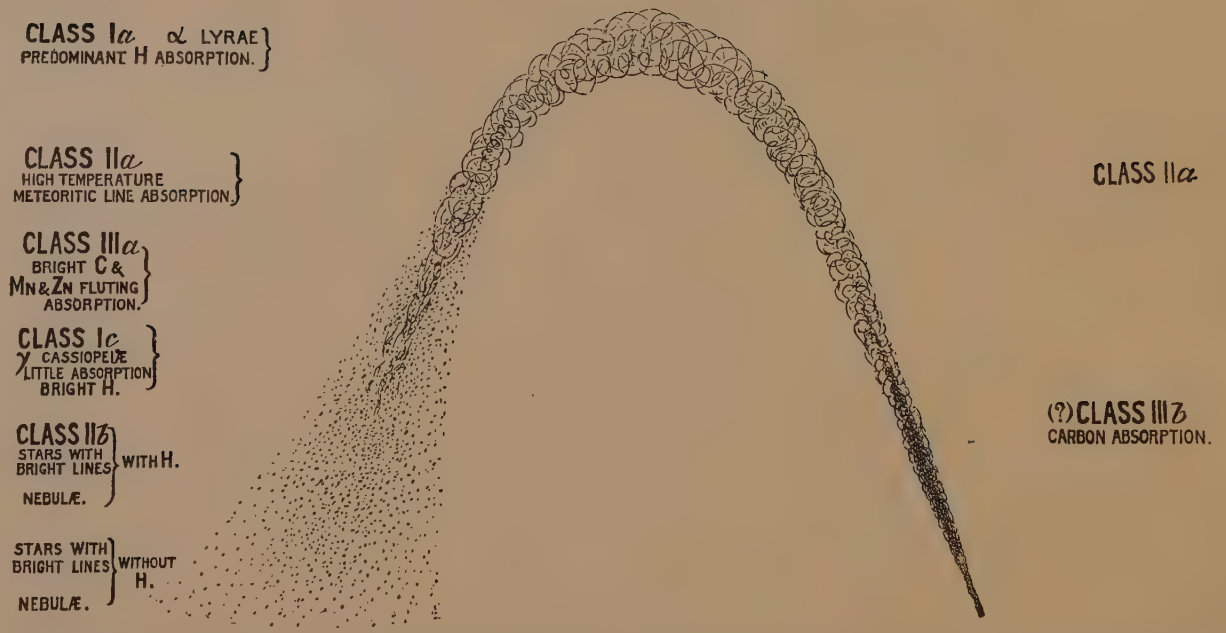
(From *Proceedings* of the Royal Society, Vol. 44, 1888.)

Having, then, gone over the various classifications of stars according to their spectra, I now proceed to consider the question of the classification of celestial bodies from a more advanced point of view. I pointed out in the year 1886 that the time had arrived when stars with increasing temperatures would require to be fundamentally distinguished from those with decreasing temperatures, but I did not then know that this was so easy to accomplish as it now appears to be; and, as I have already stated, when we consider the question of classification at all, it is neither necessary nor desirable that we should limit ourselves to the stars; we must include the nebulæ and comets as well. Stellar variability should not introduce any difficulties, seeing that as a rule in its extremest form it is the passage from one spectrum to another, even if of a different type, owing to sudden changes of temperature.

In the first classification on these lines, which is certain to be modified as our knowledge gets more exact, it is desirable to keep the groups as small in number as possible; the groups being subsequently broken up into sub-groups, or, even into species, as the various minute changes in spectra brought about by variations of temperature are better made out.

For the purpose of making clear what follows, I here introduce the "temperature curve," on which is shown the distribution of nebulæ, comets, and of stars as divided into classes by Vogel, on the two arms of the curve.

[1] Sir Joseph Norman Lockyer (1836–1920), pioneer English astrophysicist. He made important advances in the field of solar and stellar physics, and is responsible for naming the element helium, for emphasizing the two-branch theory of stellar evolution, and (jointly with Janssen) for the method of observing solar prominences without an eclipse. Lockyer's scheme of stellar development has been modified in many important details, but its basic principle, of both rising and falling temperatures in the life history of a star, is the central idea of our present theories of stellar evolution.



Temperature curve, showing the relative temperatures of the different orders of celestial bodies. The top of the curve represents the highest temperatures, and the bottom of each arm the lowest. On the left arm, the temperatures are increasing, on the right they are decreasing. The diagram shows the relative temperatures of Vogel's classes.

On one arm of this we have those stages in the various heavenly bodies in which in each case the temperature is increasing, while on the other arm we have that other condition in which we get first vaporous combination, and then ultimately the formation of a crust due to the gradual cooling of the mass, in dark bodies like, say, the companion of Sirius. At the top we of course have that condition in which the highest temperature must be assumed to exist.

To begin, then, a more general classification with the lowest temperatures, it is known that the nebulæ and comets are distinguished from most stars by the fact that we get evidence of radiation alone, or almost alone so far as we know. Absorption has been suspected in the spectra of some nebulæ, and has been observed beyond all doubt in some comets. But there are some stars in which we also get radiation, accompanied by certain absorption phenomena. But there is no difficulty in showing that nebulæ and comets are more special on account of their bright lines than on account of their absorption bands. I have already shown that in all probability the stars with bright lines are most closely allied with nebulæ. Indeed, it seems as if they are very nearly akin to those condensations in nebulæ, showing an undoubted olivine and hydrogen spectrum, which gave them the appearance of resolvability. It seems, also, highly probable that future observations with instruments of great light-collecting power, will show that in nebulæ, the spectra of which are recorded as continuous, lines including the remnants of some of the carbon flutings, which there is good reason to believe have already been traced in the spectra of bright line stars, are also present. From this point of view, the various recorded observations of regions of different colour in certain nebulæ acquire an additional interest. It is also clear that since the only real difference between comets and other meteor swarms of equal denseness is that the former are in motion round the centre of our system, comets whether at aphelion or at perihelion will fall into this group. We may, therefore, form the first group of bodies which are distinguished by the presence of bright lines or flutings in the spectrum.

The great distinction between the first group and the second would be that evidences of absorption now become prominent, and side by side with the bright flutings of carbon and occasionally the lines of hydrogen we have well-developed fluting absorption.

The second group, therefore, is distinguished from the first by mixed flutings as well as lines in the spectrum.

The passage from the second group to the third brings us to those bodies which are increasing their temperature, in which radiation and fluting absorption have given place to line absorption.

At present, the observations already accumulated have not been discussed in such a way as to enable us to state very definitely the exact retreat of the absorption—by which I mean the exact order in which the absorption lines fade out from the first members to the last in the group. We know, generally, that the earlier bodies will contain the line absorption of those substances of which we get a paramount fluting absorption in the prior group. We also know, generally, that the absorption of hydrogen will increase while the other diminishes.

The next group—the fourth—brings us to the stage of highest temperature, to stars like α Lyræ; and the division between this group and the prior one must be more or less arbitrary, and cannot at present be defined. One thing, however, is quite clear, that no celestial body without all the ultra-violet lines of hydrogen discovered by Dr. Huggins can claim to belong to it.

We have now arrived at the culminating point of temperature, and now pass to the descending arm of the curve. The fifth group, therefore, will contain those bodies in which the hydrogen lines begin to decrease in intensity, and other absorptions to take place in consequence of reduction of temperature.

One of the most interesting problems of the future will be to watch what happens in bodies along the descending scale, as compared with what happens to the bodies in Group III, on the ascending one. But it seems fair to assume that physical and chemical combinations will now have an opportunity of taking place, thereby changing the constituents of the atmosphere; that at first with every decrease of temperature an increase in the absorption lines may be expected, but it will be unlikely that the coolest bodies in this group will resemble the first one in Group III.

The next group—the sixth—is Secchi's type IV, and Vogel's Class III*b*, its distinct characteristics being the absorption flutings of carbon. The species of which it will ultimately be composed are already apparently shadowed forth in the map which accompanies Dunér's volume, and they will evidently be subsequently differentiated by the gradual addition of other absorptions to that of carbon, while at the same time the absorption of carbon gets less and less distinct.

To sum up, then, the classification I propose consists of the following groups:

Group I.—Radiation lines and flutings predominant. Absorption beginning in the last species.

Group II.—Mixed radiation and absorption predominant.

Group III.—Line absorption predominant, with *increasing* temperature. The various species will be marked by increasing simplicity of spectrum.

Group IV.—Simplest line absorption predominant.

Group V.—Line absorption predominant, with *decreasing* temperature. The various species will be marked by decreasing[1] complexity of spectrum.

Group VI.—Carbon absorption predominant.

Group VII.—Extinction of luminosity.

It will be seen from the above grouping that there are several fundamental departures from previous classifications, especially that of Vogel.

The presence of the bright flutings of carbon associated with dark metallic flutings in the second group, and the presence of only absorbing carbon in the sixth, appears to be a matter of fundamental importance, and to entirely invalidate the view that both groups (the equivalents of III*a* and III*b* of Vogel) are produced from the same mass of matter on cooling.

This point has already been dwelt upon by Pechüle.

Another point of considerable variation is the separation of stars with small absorption into such widely different groups as the first and fourth, whereas Vogel classifies them together on the ground of the small absorption in the visible part of the spectrum. But that this classification is unsound is demonstrated by the fact that in these stars, such as γ Cassiopeiæ and β Lyræ, we have intense variability. We have bright hydrogen lines instead of inordinately thick dark ones; and on other grounds, which I shall take a subsequent opportunity of enlarging upon, it is clear that the physical conditions of these bodies must be as different as they pretty well can be.

It will be seen that, with our present knowledge, it is very difficult to separate those stars the grouping of which is determined by line absorption into the Groups III and V, for the reason that so far, seeing that only one line of temperature, and that a descending one, has been considered, no efforts have been made to establish the necessary criteria. I noted this point in the paper to which I have already referred in connexion with the provisional curve.

[1] Presumably this should read "increasing complexity."]

BREDIKHINE[1]

On Comets and Meteors

(Translated from the *Annals* of the Observatory of Moscow, Series II, Vol. II, 1888.)

Concerning the Origin of Shooting Stars.—According to my investigations, abnormal comæ must consist of particles relatively too large and too heavy to be carried in normal comæ. The particles are moved by the ordinary Newtonian law of force; they have only received an *impulsion*, a "shock" (towards the sun) from the emanations which, in a normal coma, are ejected with a fairly great *repulsive* force. Now in many comets these emanations can act upon such particles without producing every time an abnormal appendage sufficiently dense and clear to become accessible for observation. Visibility is here only an accidental circumstance depending upon the abundance of the erupted particles, and it must be admitted that eruptions of this kind present a more frequent and general process taking place, under certain conditions, in a *multitude* of comets which have in the past been in the neighborhood of the Sun.

Therefore, not only abnormal comets, but in general the ejections of the *particles* towards the Sun can be regarded as the sources of shooting stars.

It is easy to show by calculation that the eruptions coming from a comet with a parabolic orbit can be the source of meteoric streams, with yearly repetitions. It is but a matter of carefully examining the properties of the orbits of the particles which, at a certain point of the parabolic orbit of the nucleus, have received some shocks, some impulsions of a given numerical value. It is clear, *a priori*, that these orbits will in general be ellipses and hyperbolas, the parabola always being only a particular case. Now, the hyperbolic trajectories carry the particles into the infinity of space, and consequently they are irrelevant to our ques-

[1] Theodor Bredikhine (1831–1904), Russian astronomer, director of the Moscow Observatory and later of the Observatory at Pulkova. His most significant work dealt with the forms of comets.

tion; therefore, it remains for us to examine the cases in which the parabola of the nucleus is changed to ellipses. In order to make these ideas concrete, let us take an average comet, that is to say, a comet whose perihelion distance $q = 0.5$.

The direction of impulsion makes with the radius vector of the nucleus the angle I.

The mean value of g (the initial speed) common to all three types is $g = 0.1$ (2950 meters per second). It is natural to admit that the initial speed of the matter pertaining to the tail is not entirely transmitted to the particles, that is to say, that the numerical value of the impulsion j is always smaller than g, but we shall first have in view a *qualitative* rather than a *quantitative* examination, and consequently we have the right to adopt in our abstract calculations the value $j = 0.1$.

The calculations are made with the aid of the following formulæ:

$$H^2 = \frac{2}{r} \qquad \beta = 90^\circ - \frac{v}{2}$$

$$H_1^2 = H^2 + j^2 - 2Hj \cos(\beta - I)$$

$$\sin \gamma = j \sin(\beta - I) : H_1$$

$$\beta_1 = \beta + \gamma \qquad m = H_1^2 r$$

$$A = r : (2 - m) \qquad T = A^{3/2}$$

$$P = m \sin^2 \beta_1 r \qquad E^2 = (A - P) : A$$

$$\cos V = (P - r) : rE \qquad Q = P : (1 + E)$$

$$V - v = \psi$$

Where:

v is the true anomaly of the nucleus at the moment of the eruption.

r radius vector of the nucleus.

β the angle of r with the tangent to the orbit.

H the speed of the nucleus.

V the angle between the radius r and the axis of the orbit of the particle.

Q the perihelion distance of the particle.

P the semi-parameter of the orbit of the particle.

E the eccentricity of this orbit.

A the semi-major axis of the orbit.

[H_1 the final speed of a particle.]

T the time of revolution of the particle around the sun expressed in years.

ψ the angle between the axes of the orbits of the nucleus and of the particle.

For every point in the orbit of the nucleus we obtain in the plane of the orbit a whole series of elliptical orbits intersecting at this point. These elliptical orbits are placed near the parabolic orbit, and differ among themselves, by their periodic times. The progression of these times is such that at the end of an interval of several years elapsed since the passage of the nucleus at perihelion, the Earth, in passing by the points where $r = 1$ will be able to meet there every year a swarm of particles in their return to the neighborhood of this point . . . Otherwise stated, the progression of the times of revolution *implies* the possibility of annual encounters of the Earth with the meteors emitted by a comet which has long since been far distant from the Sun in its parabolic orbit . . . The orbits forming the meteor shower are not at all parallel to one another; consequently the radiant of the meteors will never be reduced to a single point in the sky, but must encompass an entire area, more or less considerable . . .

Concerning the Origin of Periodic Comets.—We have already tried to study with care the origin and the development of the comæ of comets and we have succeeded in determining the numerical values of the forces and the initial speeds which are involved in this phenomenon. We have also shown how these initial speeds or impulsions applied to the particles of comets suffice to produce swarms of meteors while scattering the mass of the comet in space.

Now, observation shows us that the disintegration of comets can be affected upon a more considerable scale; I mean the division of a comet in parts more or less great.

Thus Biela's comet was divided into two independent comets; the comet of Lia (1860), observed after its passage at perihelion, appeared double; the comet of 1882 II subdivided itself after the passage at perihelion, and presented five distinct centers of condensation; besides this, in the neighborhood of the principal comet there existed other secondary ones which were seen at different times by different observers . . .

In the coma of the comet of 1882 II were observed some condensations (clouds of Schmidt) whose origin corresponded to the moments of the division of the nucleus of the comet, and which confirm the idea that this division is due to an eruptive process.

The secondary nuclei have been forced to abandon the orbit of the principal nucleus and to follow the trajectories which differ from it more or less according to the intensity of the repulsion j.

In the striking resemblances of the orbits of the comets of 1843 I, 1880 I, and 1882 II, one can see the result of the detachment of their nuclei from the nucleus of some other great comet having almost the same perihelion distance . . . Now, the periodic time of the last comet (1882 II), according to M. Kreutz, is 772 years, and while supposing that the three comets are produced by the division of the nucleus of one and the same comet, it must be admitted that the periodic times of the first two comets are: 732.5 years and 769.4 years. The generating parabolic comet must have passed its perihelion about the year 1110. Assuming that the repulsion has taken place after perihelion, before the anomaly $v = 90°$, one can calculate the value of j which would have sufficed to produce our three elliptical comets. For this we have formulæ which are in my note "Concerning the Origin of Shooting Stars."

The values of j for the three comets are, respectively: 0.0008, 0.0007, 0.0008, which gives me for the value of the *impulsion* only 21 meters per second . . .

The action of Jupiter in the transformation of orbits of comets passing not far from it is shown in several cases. On the other hand, doubling of the nucleus of the comet also leads to the transformation of its orbit, and a like doubling is also a fact shown by observation.

Thus, in the origin of periodic comets, these two agents must be kept in view. It is impossible to determine the relative preponderance of both these actions. Once the comet has become periodic, especially with an inconsiderable time of revolution, the action of Jupiter tends to shorten its major axis, while at the same time lengthening its perihelion distance. A very simple construction of a graph will be sufficient to show the necessity of these two changes. One must still note that, other things being equal, the same numerical result in these changes requires much less time for a comet with direct motion than for a comet with retrograde motion . . . In going over the list of comets, we find an entire family whose members present a remarkable resemblance in all their elements; these are the comets: Lexell, Vico, D'Arrest, Tempel_2, and the comet of 1678, if this last is not identical with one of the others. It seems almost evident that these are produced by a single comet, and indeed by means of eruptive decomposition. One can almost say the same thing in connection with the comets of 1672, Pons-Brooks and Tuttle.

HILL[1]

Introduction to a New Theory of Jupiter and Saturn

(From *Astronomical Papers* of the American Ephemeris, 1890.)

Jupiter and Saturn must have presented to the earliest observers of the celestial motions less difficulties than the interior planets. The first things noted, undoubtedly, were, that the first made a circuit of the heavens in about twelve and the second in about thirty years. Then the retrograde motion, at the time of opposition, and its extent would be perceived. The slowness and steadiness of the motion would naturally suggest the hypothesis of circular motion, but it was certainly reserved for a later and more philosophic age to explain the later-observed phenomenon by means of an epicycle.

The earliest tables of the motions of Jupiter and Saturn, as well as those of the other large planets which have come down to us, are those contained in the "Syntaxis" of *Claudius Ptolemy*. The annual parallax is there taken into account by one epicycle and the proper eccentricity of the orbit by a second. This, in the main, is the character of all the tables of the planets until the publication of *Kepler's* Tabulæ Rudolphinæ in 1627, where, for the first time, the equation of the center is derived from an elliptic formula, and we pass from heliocentric to geocentric positions in the modern way. From *Kepler* onwards the fact of the deviation of Jupiter and Saturn from a purely elliptic theory was recognized. Many attempts were made to better the theory; but it was found that no observations, embracing a long period of time, could be satisfied by elliptic elements varying proportionally to the time. *Halley* seems to have been the most successful in his tables; he adopted terms in the mean longitudes varying as the square of the time.

It was not until 1748 that any computation of the perturbations of Jupiter and Saturn, in accordance with the theory of gravitation,

[1] George William Hill (1838–1914), American theoretical astronomer, developed new methods in lunar theory and investigated particularly the mutual perturbations of Jupiter and Saturn.

was undertaken. This was by *Euler*. He appears to have limited himself to the terms which have the mean elongation of the planets from each other as their argument. Later the terms factored by the simple power of the eccentricities were added by himself, *Lalande*, *Lagrange*, *Bailly* and *Lambert*. But these terms not bringing about a reconciliation between observation and theory, *Lagrange* and *Laplace* were led to make their notable researches on the possibility of secular equations in the mean motions of the planets. At length the whole difficulty with Jupiter and Saturn was removed by *Laplace's* discovery of the great inequalities in 1786.

Delambre almost immediately constructed tables for these planets which far exceeded in accuracy any previously possessed. They are those which appear in the third edition of *Lalande's* "Astronomie." This great success seems to have stirred up *Laplace* and his colaborers to pushing the approximations still further. On the publication of the third volume of the "Mécanique Céleste," terms of the fifth order with respect to the eccentricities and mutual inclination, as well as some of two dimensions with respect to disturbing forces, had been added to the coefficients of the great inequalities. That these advances might be utilized *Bouvard* constructed tables of the planets founded on observed oppositions from 1747 to 1803. The formulæ used are very nearly those given in the "Mécanique Céleste," Tom. III. These tables were published by the Bureau des Longitudes in 1808. It was discovered, however, that the terms of the fifth order, mentioned above, had been taken with the wrong sign. This led *Bouvard* to prepare a new edition of his tables, which appeared in 1821, and in which this error was rectified, and the observations employed in the discussion extended to 1814. Although *Bouvard* himself speaks in admiration of the small residuals shown by the comparison of his theory with the observations, yet a glance shows their tendency to a systematic character, and this, too, with observations rather rudely reduced.

Plana undertook, shortly after, to compute the portions of the great inequalities which arise from considering the square of the disturbing force. The results he obtained failed to satisfy an equation of condition which *Laplace* had employed in his investigation. After some discussion *Laplace* abandoned his equation and substituted for it another, which *Plana's* results were as far from satisfying as before. *Pontécoulant* then, taking up the subject,

discovered that *Laplace's* results had been taken with the wrong sign, and that *Plana* had made errors of some importance in his investigation. When these oversights had been corrected the different results were brought into tolerable agreement.

However, the failure of *Bouvard's* tables to better represent observations, and his getting for the mass of Jupiter a value so much smaller than was shortly after obtained from the action of this planet on the asteroids and on its own satellites, can not be explained by this error of sign. It is somewhat singular that no one has yet pointed out the real cause, which, it seems, must be either some error in the coefficients of his formulæ or some error in putting his equations in tables.

Neither *Laplace's*, *Plana's*, nor *Pontécoulant's* determination of these second-order terms can be regarded as anything else than a very rude and inadequate approximation.

Hansen had, a short time previous, imagined a new method of treating perturbations. In the "Mécanique Céleste," *Laplace* had determined all long-period inequalities as if they were to be applied to the mean longitude, and had so directed they should, while the short-period ones were derived as if they were to be added to the true longitude. There is, therefore, a want of congruity, and even of rigor, in this way of proceeding. For *Laplace* has nowhere shown how these two modes of application can be employed in unison. It is plain there would be as many methods of perturbations as there were opinions as to the dividing line separating long from short-period inequalities. These imperfections no doubt attracted the attention of *Hansen*, whose thought must have been: Since it is advantageous to apply the long-period terms to the mean longitude, and indifferent whether the short-period ones are applied to the mean or true, why not apply all to the mean, and, moreover, compute the radius-vector and latitude with this equated quantity? Then the additional quantities necessary to complete the values of the latter co-ordinates would be, for the first, a function of three variations of the elements, and for the second, a function of two only. This, undoubtedly, was the origin of *Hansen's* new method.

He determined to apply it to Jupiter and Saturn, and his memoir, crowned by the Berlin Academy, must be regarded as the earliest example of an adequate treatment of perturbations of the second order with respect to disturbing forces. In all previous investigations it is impossible to form a conception of the probable

magnitude of the terms passed over on account of the habit of the investigator of selecting here and there a term to be computed. But in *Hansen* the continuity in the computed terms enables one to form a fair judgment as to the importance of those neglected. However, Saturn alone is treated with a fair degree of completeness. The expressions for Jupiter are limited to the terms arising from the first power of the disturbing force. Had this theory of Saturn been completed by the addition of the terms due to the action of Uranus and the whole compared with the observations more carefully reduced, as they then could have been by the aid of *Bessel's* "Tabulæ Regiomontanæ," very excellent tables would have been obtained. But *Hansen* seems to have been carried away with the ambition of applying his peculiar method of treatment to the lunar theory.

A long period of over forty years now elapsed without anything being contributed to the theories of Jupiter and Saturn, for the expressions of the perturbations given in *Pontécoulant's* "Théorie Analytique du Systéme du Monde," beyond the correction of the error of sign in the second-order terms of the great inequalities, do not seem to be in anything more perfect than those found in the Mécanique Céleste . . .

Hansen, in 1875, published a memoir on Jupiter. But here, deserting his earlier notions on the lack of convergence in algebraical developments, he confines himself to calculating the easier terms of the co-ordinates. Hence this memoir can not be regarded as advancing much our knowledge of the subject.

In the years 1874 to 1876 appeared *Leverrier's* investigation, concluding with the tables which are at present employed for all the European ephemerides. The method followed is that of attributing the perturbations to the six elements of the Keplerian ellipse; and, contrary to the mode followed in his earlier planetary theories, these are also the quantities tabulated . . .

The desirableness of a new investigation of the subject has been generally admitted, and fault has been found with the amount of labor required to deduce positions of the planets from *Leverrier's* tables. But I had not these inducements to take up the subject when I began work, for these tables were then unpublished. The long interval which occurred between the publication of *Leverrier's* theory of Mars and the appearance of anything from him on Jupiter and Saturn was the occasion of leading me to consider the under-

taking. On making known to the Superintendent of the American Ephemeris my desire to take up the problem I was relieved from all other routine work, and supplied with the assistance necessary to duplicate all my computations which required this safeguard against error. It was desired to abandon the use of the antiquated tables of *Bouvard*, and it appeared uncertain when *Leverrier* would publish his.

The plan, therefore, was to form theories of Jupiter and Saturn which would be practically serviceable for a space of three hundred years on each side of a central epoch taken near the center of gravity of all the times of observation; theories whose errors in this interval would simply result not from neglected terms in the developments, but from the unavoidable imperfections in the values of the arbitrary constants and masses adopted from the indications of observation.

Such were the considerations which influenced the adoption of the course to be followed. As there was no desire to lose time by forming a special method of treatment for the problem in hand it was decided to employ the method of *Hansen*, with such slight modifications as the exigencies of the case might suggest. On account of the presence of the great inequalities this method seemed to me to give expressions best suited to tabulation. The latest form of this method appears in *Hansen's* memoir entitled "Auseinandersetzung," etc. The employment of the eccentric anomaly of the planet whose co-ordinates are sought as the independent variable undoubtedly augments the convergence of the series; but the adoption of this mode of proceeding would bring about the use of two independent variables, one of the co-ordinates of Jupiter, another for those of Saturn. As the developments have to be pushed to terms of three dimensions with respect to disturbing forces the heaviest part of the labor consists in forming products of periodic series, one of which belongs to Jupiter, the other to Saturn; and as integration can not be performed unless these products are transformed so as to involve but one variable we should have an endless series of transformations to make. It, therefore, seems a necessity to have a single independent variable for the whole work. In consequence, the final form adopted for all the periodic series is in terms of the mean anomalies, so that the time is always the independent variable. Fortunately, very slight and readily perceived changes only are necessary in the formulæ of the "Auseinandersetzung" to render them applicable to the modified mode of proceeding.

E. C. PICKERING[1]

Early Work on Stellar Spectra

(From "Stellar Photography," 1886, *Memoirs* of the American Academy of Arts and Sciences, Vol. 11.)

An investigation of the photographic spectra of the stars was conducted on an entirely different method from that employed by previous investigators . . . A large prism was constructed, and placed in front of the object-glass, as was first suggested and tried by Father Secchi in his eye observations of stellar spectra.

The great advantages of this method are, first, that the loss of light is extremely small, and, secondly, that the stars over the entire field of the instrument will impress their spectra upon the plate. As a result, while previous observers have succeeded in photographing the spectrum of but one star at a time, and have not obtained satisfactory results from stars fainter than the second or third magnitude, we have often obtained more than a hundred spectra on a single plate, many of them relating to stars no brighter than the seventh or eighth magnitude.

The first experiments were made in May, 1885, placing a 30° prism in front of the object-glass of the lens . . . No clockwork was used, the spectra being formed of the trails of the stars. In the spectrum of the Pole-star over a dozen lines could be counted. In the spectrum of α *Lyræ* the characteristic lines were shown very clearly. Exposures of two or three minutes were usually employed, although one minute gave an abundant width. In the spectrum of α *Aquilæ*, besides the lines seen in α *Lyræ*, some of the additional faint lines noticed by Dr. Draper were certainly seen.

In the autumn of 1885, two prisms were constructed, having clear apertures of 20 cm. and angles of about 5° and 15°. They

[1] Edward Charles Pickering (1846–1919), American astronomer, fourth director of the Harvard College Observatory, measured personally the majority of the magnitudes in the Harvard Photometries. He inaugurated extensive investigations in photographic photometry, variable stars, and spectroscopy, and devised many of the instruments necessary for these researches.

could be placed over the object-glass of the photographic telescope without reducing the aperture. The second of these prisms was that actually employed in the experiments described below.

The prism was always placed with its edges horizontal when the telescope was in the meridian. The spectrum then extended north and south. If clockwork was attached, a line of light would be formed too narrow to show the lines of the spectrum satisfactorily. The usual method of removing this difficulty is the employment of a cylindrical lens to widen the spectrum; but if the clockwork is disconnected, the motion of the star will produce the same effect. Unless the star is very bright, the motion will, however, be so great that the spectrum will be too faint. It is only necessary to vary the rate of the clock in order to give any desired width to the spectrum. A width of about one millimetre is needed to show the fainter lines. This distance would be traversed by an equatorial star in about twelve seconds. The longest time that it is ordinarily convenient to expose a plate is about an hour. If then the clock is made to gain or lose twelve seconds an hour, it will have the rate best suited for the spectra of the faintest stars. A mean-time clock loses about ten seconds an hour. It is only necessary to substitute a mean-time clock for the sidereal clock to produce the required rate. It was found more convenient, however, to have an auxiliary clock whose rate could be altered at will by inserting stops of various lengths under the bob of the pendulum. One of these made it gain twelve seconds in about five minutes, the other produced the same gain in an hour. The velocity of the image upon the plate when the clock is detached could thus be reduced thirty or three hundred and sixty times. This corresponds to a difference of 3.7 and 6.1 magnitudes respectively. Since the spectrum of a star of the second magnitude could be taken without clockwork, stars of the sixth and eighth magnitudes respectively could be photographed equally well with the arrangement described above . . .

Photographs were also taken of the spectra of the fainter stars in certain regions. The auxiliary clock was set so that it should gain about ten seconds in an hour, and a single exposure of about an hour was made upon each plate. The work of photographing the entire sky by this process proved to be too large to be undertaken by the aid of the Bache Fund. Fortunately, Mrs. Henry Draper, as a memorial to her husband, has made provision for

continuing this investigation at the Observatory of Harvard College . . .

Henry Draper Memorial.[1]—Dr. Henry Draper, in 1872, was the first to photograph the lines of a stellar spectrum. His investigation, pursued for many years with great skill and ingenuity, was most unfortunately interrupted in 1882 by his death. The recent advances in dry-plate photography have vastly increased our powers of dealing with this subject. Early in 1886, accordingly, Mrs. Draper made a liberal provision for carrying on this investigation at the Harvard College Observatory, as a memorial to her husband. The results attained are described below, and show that an opportunity is open for a very important and extensive investigation in this branch of astronomical physics. Mrs. Draper has accordingly decided greatly to extend the original plan of work, and to have it conducted on a scale suited to its importance. The attempt will be made to include all portions of the subject, so that the final results shall form a complete discussion of the constitution and conditions of the stars, as revealed by their spectra, so far as present scientific methods permit. It is hoped that a greater advance will thus be made than if the subject was divided among several institutions, or than if a broader range of astronomical study was attempted. It is expected that a station to be established in the southern hemisphere will permit the work to be extended so that a similar method of study may be applied to stars in all parts of the sky. The investigations already undertaken, and described below more in detail, include a catalogue of the spectra of all stars north of $-24°$ of the sixth magnitude and brighter, a more extensive catalogue of spectra of stars brighter than the eighth magnitude, and a detailed study of the spectra of the bright stars. This last will include a classification of the spectra, a determination of the wave lengths of the lines, a comparison with terrestrial spectra, and an application of the results to the measurement of the approach and recession of the stars. A special photographic investigation will also be undertaken of the spectra of the banded stars, and of the ends of the spectra of the bright stars. The instruments employed are an 8-inch Voigtländer photographic lens reground by Alvan Clark and Sons, and Dr. Draper's 11-inch photographic lens, for which Mrs. Draper has provided a new mounting and observatory . . .

[1] From *First Annual Report* of the Photographic Study of Stellar Spectra, University Press, Cambridge, Mass., 1887.

The First Spectroscopic Binary.[1]—In the Third Annual Report of the Henry Draper Memorial, attention is called to the fact that the *K* line in the spectrum of ζ Ursæ Majoris occasionally appears double. The spectrum of this star has been photographed at the Harvard College Observatory on seventy nights and a careful study of the results has been made by Miss A. C. Maury, a niece of Dr. Draper. The *K* line is clearly seen to be double in the photographs taken on March 29, 1887, on May 17, 1889 and on August 27 and 28, 1889. On many other dates the line appeared hazy, as if the components were slightly separated, while at other times the line appears to be well defined and single. An examination of all the plates leads to the belief that the line is double at intervals of 52 days, beginning March 27, 1887, and that for several days before and after these dates it presents a hazy appearance. The doubling of the line was predicted for October 18, 1889, but only partially verified. The line appeared hazy or slightly widened on several plates but was not certainly doubled. The star was, however, low and only three prisms could be used, while the usual number was four. The predicted times at which the line should be again double are on December 9, 1889 and on January 30, 1890. The hydrogen lines of ζ Ursæ Majoris are so broad that it is difficult to decide whether they are also separated into two or not. They appear, however, to be broader when the *K* line is double than when it is single. The other lines in the spectrum are much fainter, and although well shown when the *K* line is clearly defined, are seen with difficulty when it is hazy. Several of them are certainly double when the *K* line is double. Measures of these plates gave a mean separation of 0.246 millionths of a millimeter for a line whose wave length is 448.1, when the separation of the *K* line, whose wave length is 393.7, was 0.199. The only satisfactory explanation of this phenomenon as yet proposed is that the brighter component of this star is itself a double star having components nearly equal in brightness and too close to have been separated as yet visually. Also that the time of revolution of the system is 104 days. When one component is approaching the earth all the lines in its spectrum will be moved toward the blue end, while all the lines in the spectrum of the other component will be moved by an equal amount in the opposite direction if their masses are equal. Each line will thus be separated into two. When the motion becomes perpendicular to the line of sight the spectral lines recover their true wave length and become single. An idea of the actual

[1] From "On the Spectrum of ζ Ursæ Majoris," *American Journal of Science,* 1890.

dimensions of the system may be derived from the measures given above. The relative velocity as derived from the *K* line will be 0.199 divided by its wave length 393.7 and multiplied by the

A portion of the spectrum of the spectroscopic binary β Aurigæ, similar to ξ Ursæ Majoris, showing the doubling of the K line due to orbital motion. From a Harvard Observatory photograph.

velocity of light 186,000, which is equal to 94 miles a second. A similar calculation for the line whose wave length is 448.1 gives 102 miles per second. Since the plates were probably not taken at the exact time of maximum velocity these values should be

somewhat increased. We may, however, assume this velocity to be about one hundred miles per second. If the orbit is circular and its plane passes through the sun, the distance traveled by one component of the star regarding the other as fixed would be 900 million miles, and the distance apart of the two components would be 143 million miles, or about that of Mars and the sun. The combined mass would be about forty times that of the sun to give the required period. In other words, if two stars, each having a mass twenty times that of the sun, revolved around each other at a distance equal to that of the sun and Mars, the observed phenomenon of the periodic doubling of the lines would occur. If the orbit was inclined to the line of sight its dimensions and the corresponding masses would be increased. An ellipticity of the orbit would be indicated by variations in the amount of the separation of the lines, which will be considered hereafter. The angular distance between the components is probably too small to be detected by direct observation. The greatest separation may be about 1.5 times the annual parallax. Some other stars indicate a similar peculiarity of spectrum, but in no case is this as yet established.

ADDENDUM, Dec. 17.—The predicted doubling of the lines of ζ Ursæ Majoris on December 8th was confirmed on that day by each of three photographs. Two more stars have been found showing a similar periodicity: β Aurigæ and σ Ophiuchi (H.P. 1100 and 2909).

VARIABLE STARS

Classification and Interpretation.[1]—Variable stars may be divided into several classes, according to the nature of the fluctuations of their light. First, temporary stars, which appear suddenly, and gradually fade away during the next few months. The most famous star of this class is that observed in 1572, by Tycho Brahe. The new stars in *Corona Borealis* in 1866 and in *Cygnus* in 1876, are recent examples of this class. Second, a large part of the variable stars pass from their maximum to their minimum and back again, in from six months to two years, the period and the brightness at the maximum and minimum being somewhat variable. The change in light is generally very great, amounting to several hundred, or even thousand times. The most striking

[1] From *Proceedings* of the American Academy of Arts and Science, Vol. 16, 1881.

examples of this class are *o Ceti* and *χ Cygni*. Thirdly, we have the slight changes to which many (or, according to Dr. Gould, most) stars are liable. These changes seem to be irregular in many cases; at least, their law is not yet known. Examples of this class are furnished in *α Orionis* and *α Cassiopeiæ*. Fourthly, certain stars continually vary, going through a series of changes in the course of a few days, which appears to be repeated exactly. Two causes seem here to be superimposed, one producing one maximum and one minimum in each period, the other two maxima and two minima in the same time. As examples, *β Lyræ* and *δ Cephei* may be noted. Fifthly, we have a class of stars which during the greater part of the time remain unchanged in brightness, but at regular intervals lose in the course of a few hours a large part of their light, and regain it with equal rapidity. These changes appear to be repeated with the greatest regularity, so that the interval can be computed in some cases within a fraction of a second. *Algol*, or *β Persei*, is the most striking example of this class to which *δ Cancri* and *δ Libræ* also belong.

Various theories have been advanced to account for these phenomena. Probably different causes act in the case of the different classes. One theory would assume that by a collision, or by the liberation and ignition of a vast amount of hydrogen, the star was suddenly heated to incandescence, and gradually lost its light by cooling. This explanation would apply only to stars of the first class; it is strengthened in the case of the new star in *Cygnus* by the observations with the spectroscope. The spectrum gave at first the lines of incandescent hydrogen which disappeared as the light faded. It has been urged that, to account for the rapid cooling, the star must have been small, perhaps only a few miles in diameter, and consequently not very distant. This view is contradicted by the absence of perceptible parallax. If we consider how quickly a meteorite becomes heated, and again gives up its heat, this argument loses its force. The star may be large and distant, the surface only being heated, and soon losing its heat by radiation and conduction. This explanation appears more probable than that the light is cut off by clouds of smoke or steam, as has been suggested by some astronomers.

Stars constituted like our Sun, but in which the variations in size of the spots would be far greater, might undergo considerable changes in light. While it is difficult to account for the great changes in class two in this way, those in class three may be thus

explained. A popular theory for the variation of stars of short period is that it is due to the revolution of the star upon its axis, when the different portions are of unequal brightness. The variation in light of Iapetus, the outer satellite of Saturn, is commonly explained in this way. A similar effect would be produced if the star was not spherical, and in revolving exposed a disk of varying area. A great variation could not thus be produced without the revolving body assuming a condition of unstable equilibrium. For the application of these principles of Iapetus, see *Annals of Harvard College Observatory*, Vol. XI, 264. This theory may explain the variations of stars of the fourth class. Another theory would account for the changes of light by an opaque body or satellite passing between the star and the observer. It will be the object of the following discussion to show how fully this explanation will account for the variations of stars of the fifth class. A modification of this theory would replace the single eclipsing body by a cloud of meteorites. Such a theory will account for almost anything by suitably modifying the distribution of the meteors. If we can show that all the effects may be explained by a single body, or what amounts to the same thing, a spherical cloud of meteors so dense as to be opaque, there seems to be no reason for assuming a cloud of another form. All that can be claimed for any theory is that it explains all the facts. If then the computed variations of light agree with the observations within the limits of errors of observation, that is all that can be asked, and the theory should be accepted as the most probable explanation until some new fact is discovered which it will not explain, or some new theory which agrees equally well with observation and appears to be less improbable. The diminution in light might be caused by the interposition of a body which was self-luminous, instead of dark. We should then have a close double-star, one component of which passed in front of the other. If the orbit was circular, we should have two minima during each revolution, and at these times the star would appear of unequal brightness, unless the intrinsic brightness of the two bodies was the same. When the darker body passed in front of the brighter, the light would be less than when the brighter passed in front of the darker. If the orbit was elliptical there might be only one minimum. In the case of Algol more than half the light is cut off at the minimum; consequently one body must be darker than the other. As no second minimum

has ever been observed, it is probable that the eclipsing body is not self-luminous.[1]

Long Period Variables.[2]—A *natural* classification of the variable stars seems to place together those having a period of one or two years. They have many points in common; for instance, when near maximum the lines in their spectra due to hydrogen are usually bright. This peculiarity has in several cases led to their discovery, and perhaps furnishes a clue to the cause of their variation in light. Their color is generally red, and the change in brightness very great. Several of them at maximum are visible to the naked eye, but at minimum become wholly invisible, or at least beyond the reach of any but the largest telescopes. This variation is as great as that between the brightest and faintest stars visible to the naked eye. Numerous observations have been made of many of these stars, but, generally, with the object of determining the times at which they attain their greatest brilliancy. The nature of the changes, or form of light curve, as it is called, has been comparatively neglected. It is the object of the present paper to provide a means of supplying this omission.

Many astronomers, provided in some cases with excellent telescopes, find difficulty in using them in such a way as will really advance astronomical science. The study of these variables seems especially adapted to such cases. Except the telescope itself, no delicate apparatus, like clock-work or micrometer, is required. Even divided circles are not essential, although they facilitate observation. The variation in brightness is also so great that even rough measures will have a value, since the laws regulating many of these variables are almost entirely unknown. When the total change in brightness is small, great skill is required to determine variations with accuracy. But less precision is needed when the variations amount to several magnitudes, especially as great accuracy seems to be unattainable owing to the color of these stars.

The best method of making the observation is that invented by Sir William Herschel, and developed by Argelander. The variable is compared with a known star of very nearly the same brightness, and the difference, if any, is estimated. If two stars of equal brightness are watched for a few seconds, the relative brightness will appear to vary. If one appears the brighter as often as the

[1 Subsequent refined measures show a shallow secondary minimum.]

[2] From "Variable Stars of Long Period," University Press, Cambridge, Mass., 1891.

other, they may be assumed to be equal. If, however, one appears brighter oftener than the other does, the difference in brightness may be regarded as one grade or step. In actual practice this difference in brightness is found to be nearly constant for different persons, and to be a little greater than one-tenth of a stellar magnitude. When one star generally appears brighter, but sometimes the stars appear equal, the difference is two steps. If one star always seems brighter than the other, while the difference always remains small, this difference may be regarded as three steps. Somewhat larger intervals may be estimated, but with less accuracy, and should be avoided as far as possible. To study the changes in light of a variable star it is only necessary to select a series of comparison stars, and compare the variable with stars of nearly the same brightness taken from this series. If possible, two comparison stars should be used, one a little brighter, the other a little fainter than the variable. Evidently the comparison stars should be near the variable, and a very low power should be used, so that the apparent distance may be small. Double stars and those near brighter stars should not be used for comparison stars, since otherwise large errors will be introduced, whose amount will vary with the instrument used. Since a star near the edge of the field of a telescope appears brighter than when near the centre, it is better to bring each star in turn into the centre, rather than to place them equally near the edge of the field. When the distance between two stars is so small that they cannot readily be observed alternately, as just recommended, it is probable that, owing to the varying sensitiveness of different portions of the retina, their relative brightness will appear to vary according to their position. The head should, therefore, always be turned until the line connecting the eyes is parallel to that connecting the stars, in order that the error may be the same in all cases. Its amount may be determined by selecting several pairs of stars such that in each pair the stars shall be nearly equal in brightness and one over the other. Compare these stars with the upper stars in the successive pairs alternately to the right and left, and repeat with the head turned the opposite way, so that each pair is measured once with the upper star to the right and once to the left. The mean of the differences of the results when the upper star is turned to the right and to the left will equal twice the error due to their position. When the variable is bright the comparison should also be made with the finder, with a field-glass, or with the unaided eye, since it is difficult to compare two very bright images.

CHANDLER[1]

Discovery of the Laws of Latitude Variation

(From *Astronomical Journal*, 1891.)

In the determination of the latitude of Cambridge with the Almucantar, about six years and a half ago, it was shown that the observed values, arranged according to nights of observation, exhibited a decided and curious progression throughout the series, the earlier values of $C - O$ being positive, the later ones negative, and the range from November, 1884, to April, 1885, being about four-tenths of a second. There was no known or imaginable instrumental or personal cause for this phenomenon, yet the only alternative seemed to be an inference that the latitude had actually changed. This seemed at the time too bold an inference to place upon record, and I therefore left the results to speak for themselves.[2] The subsequent continuation of the series of observations to the end of June, 1885, gave a negative maximum about May 1, while the discussion of the previous observations from May to November, 1884, gave a positive maximum about September 1, indicating a range of 0″.7 within a half-period of about seven months . . .

It thus appears that the apparent change in the latitude of Cambridge is verified by this discussion of more abundant material. The presumption that it is real, on this determination alone, would justify further inquiry.

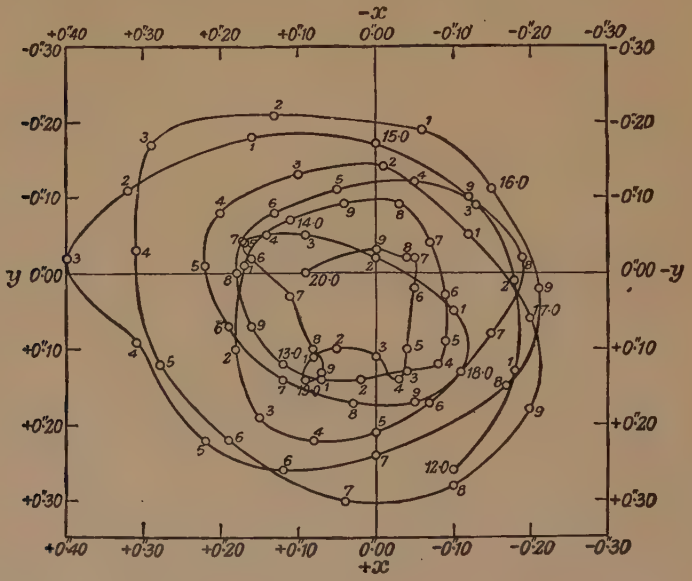
Curiously enough, Dr. *Küstner*, in his determination of the aberration, from a series of observations coincident in time with those of the Almucantar, came upon similar anomalies, and his results, published in 1888, furnish a counterpart to those which I had pointed out in 1885. The verification afforded by the recent parallel determinations at Berlin, Prague, Potsdam, and Pulkowa,

[1] Seth Carlo Chandler (1846–1913), American astronomer, is known for his three early catalogues of variable stars as well as for the epochal work, described above, on the motions of the Earth's polar axis.

[2] The detection of the variation of latitude was announced by F. K. Küstner in 1888.]

which show a most surprising and satisfactory accordance, as to the character of the change, in range and periodicity, with the Almucantar results, has led me to make further investigations on the subject. They seem to establish the nature of the law of these changes, and I will proceed to present them in due order . . .

Before entering upon the details of the investigations spoken of in the preceding number, it is convenient to say that the general result of a preliminary discussion is to show a revolution of the earth's pole in a period of 427 days, from west to east, with a



The Wandering of the Earth's Pole from 1912–1920. From Jones' *General Astronomy*. (*By permission of Edward Arnold & Co., Publishers.*)

radius of thirty feet, measured at the earth's surface. Assuming provisionally, for the purpose of statement, that this is a motion of the north pole of the principal axis of inertia about that of the axis of rotation, the direction of the former from the latter lay towards the Greenwich meridian about the beginning of the year 1890. This, with the period of 427 days, will serve to fix approximately the relative positions of these axes at any other time, for any given meridian. It is not possible at this stage of the investigation to be more precise, as there are facts which appear to show that the rotation is not a perfectly uniform one, but is subject to

secular change, and perhaps irregularities within brief spaces of time . . .

In 1862, Professor *Hubbard* began a series of observations of *α Lyræ* at the Washington Observatory with the prime-vertical transit-instrument, for the purpose of determining the constants of aberration and nutation and the parallax of the star. The methods of observation and reduction were conformed to those used with such success by *W. Struve*. After *Hubbard's* death the series was continued by Professors *Newcomb*, *Hall* and *Harkness* until the beginning of 1867. Professor *Hall* describes these observations as the most accurate determinations of declination ever made at the Naval Observatory. The probable error of a declination from a single transit was $\pm 0''\cdot 141$, and, judging from the accidental errors, the series ought to give trustworthy results. Upon reducing them, however, it was found that some abnormal source of error existed, which resulted in anomalous values of the aberration-constant in the different years, and a negative parallax in all. A careful verification of the processes of reduction failed to discover the cause of the trouble, and Professor *Hall* says that the results must stand as printed, and that probably some annual disturbance in the observations or the instrument occurred, which will never be explained, and which renders all deductions from them uncertain. The trouble could not be connected with personal equation, the anomalies remaining when the observations of the four observers who took part were separately treated. Nor, as Professor *Hall* points out, will the theoretical ten-month period in the latitude furnish the explanation.

It is manifest, however, that if the 427-day period exists, its effect ought to appear distinctly in declination-measurements of such high degree of excellence as these presumably were, and, as I hope satisfactorily to show, actually are. When this variation is taken into account, the observations will unquestionably vindicate the higher expectations entertained with regard to them by the accomplished and skilful astronomers who designed and carried them out.

SCHIAPARELLI[1]

The Planet Mars

(From "The Planet Mars," 1893; translation by Wm. H. Pickering, *Astronomy and Astro-Physics*, Vol. 13, 1894.)

The Polar Caps.—Many of the first astronomers who studied Mars with the telescope, had noted on the outline of its disc two brilliant white spots of rounded form and of variable size. In process of time it was observed that whilst the ordinary spots upon Mars were displaced rapidly in consequence of its daily rotation, changing in a few hours both their position and their perspective, that the two white spots remained sensibly motionless at their posts. It was concluded rightly from this, that they must occupy the poles of rotation of the planet, or at least must be found very near to them. Consequently they were given the name of *polar caps* or *spots*. And not without reason is it conjectured, that these represent upon Mars that immense mass of snow and ice, which still today prevents navigators from reaching the poles of the Earth. We are led to this conclusion not only by the analogy of aspect and of place, but also by another important observation . . .

As things stand, it is manifest, that if the above mentioned white polar spots of Mars represent snow and ice, they should continue to decrease in size with the approach of summer in those places, and increase during the winter. Now this very fact is observed in the most evident manner. In the second half of the year 1892, the southern polar cap was in full view; during that interval, and especially in the months of July and August, its rapid diminution from week to week was very evident, even to those observing with common telescopes. This snow (for we may well call it so), which in the beginning reached as far as latitude 70°, and formed a cap of over 2000 kilometers (1200 miles) in diameter, progressively diminished, so that two or three months later little more of it

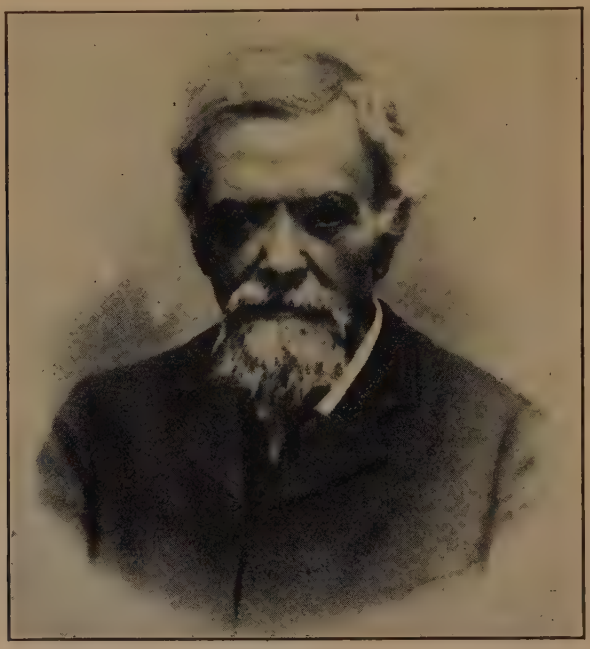
[1] Giovanni Virginio Schiaparelli (1835–1910), Italian astronomer, director of the Royal Observatory of Milan. He discovered the relation of the Perseid meteors to the Comet 1862 III, and was the founder of Martian topography.

remained than an area of perhaps 300 kilometers (180 miles) at the most, and still less was seen later in the last days of 1892. In these months the southern hemisphere of Mars had its summer; the summer solstice occurring upon October 13. Correspondingly the mass of snow surrounding the northern pole should have increased; but this fact was not observable, since that pole was situated in the hemisphere of Mars which was opposite to that facing the Earth. The melting of the northern snow was seen in its turn in the years 1882, 1884 and 1886.

These observations of the alternate increase and decrease of the polar snows are easily made, even with telescopes of moderate power, but they become much more interesting and instructive when we can follow assiduously the changes in their more minute particulars, using larger instruments. The snowy regions are then seen to be successively notched at their edges; black holes and huge fissures are formed in their interiors; great isolated pieces many miles in extent stand out from the principal mass, and dissolving, disappear a little later. In short, the same divisions and movements of these icy fields present themselves to us, at a glance, that occur during the summer of our own arctic regions, according to the descriptions of explorers . . .

Travellers in the arctic regions have frequent occasion to observe how the state of the polar ice at the beginning of the summer, and even at the beginning of July is always very unfavorable to their progress. The best season for exploration is in the month of August, and September is the month in which the trouble from the ice is the least. Thus in September our Alps are usually more practicable than at any other season. And the reason for it is clear, the melting of the snow requires time, a high temperature is not sufficient, it is necessary that it should continue, and its effect will be so much the greater, as it is the more prolonged. Thus, if we could slow down the course of our seasons, so that each month should last sixty days instead of thirty, in the summer in such a lengthened condition, the melting of the ice would progress much further, and perhaps it would not be an exaggeration to say that the polar cap at the end of the warm season would be entirely destroyed. But one can not doubt in any case, that the fixed portion of such a cap would be reduced to much smaller size, than we see it to-day. Now this is exactly what happens in Mars. The long year, nearly double our own, permits the ice to accumulate during the polar night of ten or twelve months, so as to descend

in the form of a continuous layer as far as parallel 70°, or even further. But in the day which follows of twelve or ten months, the Sun has time to melt all, or nearly all, of the snow of recent formation, reducing it to such a small area, that it seems to us no more than a very white point. And perhaps this snow is entirely destroyed, but of this there is at present no satisfactory observation . . .



Schiaparelli.

As has been stated, the polar snows of Mars prove in an incontrovertable manner, that this planet, like the Earth, is surrounded by an atmosphere capable of transporting vapor from one place to another. These snows are in fact precipitations of vapor, condensed by the cold, and carried with it successively. How carried with it, if not by atmospheric movement? The existence of an atmosphere charged with vapor has been confirmed also by spectroscopic observations, principally those of Vogel; according to which this atmosphere must be of a composition differing little from our own, and above all *very rich in aqueous vapor*. This is a fact of the highest importance, because from it we can rightly affirm with

much probability, that to water, and to no other liquid is due the seas of Mars and its polar snows. When this conclusion is assured beyond all doubt, another one may be derived from it, of not less importance—that the temperature of the Arean climate, notwithstanding the greater distance of that planet from the Sun, is of the same order as the temperature of the terrestrial one. Because, if it were true, as has been supposed by some investigators, that the temperature of Mars was on the average very low (from 50° to 60° below zero!), it would not be possible for water vapor to be an important element in the atmosphere of that planet, nor could water be an important factor in its physical changes; but would give place to carbonic acid, or to some other liquid whose freezing point was much lower . . .

The "Canals" of Mars.—All the vast extent of the continents is furrowed upon every side by a network of numerous lines or fine stripes of a more or less pronounced dark color, whose aspect is very variable. These traverse the planet for long distances in regular lines, that do not at all resemble the winding courses of our streams. Some of the shorter ones do not reach 500 kilometers (300 miles), others on the other hand extend for many thousands, occupying a quarter or sometimes even a third of a circumference of the planet. Some of these are very easy to see, especially that one which is near the extreme left-hand limit of our map, and is designated by the name of Nilosyrtis. Others in turn are extremely difficult, and resemble the finest thread of spider's web drawn across the disc. They are subject also to great variations in their breadth, which may reach 200 or even 300 kilometers (120 to 180 miles) for the Nilosyrtis, whilst some are scarcely 30 kilometers (18 miles) broad.

These lines or stripes are the famous canals of Mars, of which so much has been said. As far as we have been able to observe them hitherto, they are certainly fixed configurations upon the planet. The Nilosyrtis has been seen in that place for nearly one hundred years, and some of the others for at least thirty years. Their length and arrangement are constant, or vary only between very narrow limits. Each of them always begins and ends between the same regions. But their appearance and their degree of visibility vary greatly, for all of them, from one opposition to another, and even from one week to another, and these variations do not take place simultaneously and according to the same laws for all, but in most cases happen apparently capriciously, or at

least according to laws not sufficiently simple for us to be able to unravel. Often one or more become indistinct, or even wholly invisible, whilst others in their vicinity increase to the point of becoming conspicuous even in telescopes of moderate power. The first of our maps shows all those that have been seen in a long series of observations. This does not at all correspond to the appearance of Mars at any given period, because generally only a few are visible at once.

Every canal (for now we shall so call them), opens at its ends either into a sea, or into a lake, or into another canal, or else into the intersection of several other canals. None of them have yet been seen cut off in the middle of the continent, remaining without beginning or without end. This fact is of the highest importance. The canals may intersect among themselves at all possible angles, but by preference they converge towards the small spots to which we have given the name of lakes. For example, seven are seen to converge in Lacus Phœnicis, eight in Trivium Charontis, six in Lunæ Lacus and six in Ismenius Lacus . . .

That the lines called canals are truly great furrows or depressions in the surface of the planet, destined for the passage of the liquid mass, and constituting for it a true hydrographic system, is demonstrated by the phenomena which are observed during the melting of the northern snows. We have already remarked that at the time of melting they appeared surrounded by a dark zone, forming a species of temporary sea. At that time the canals of the surrounding region become blacker and wider, increasing to the point of converting, at a certain time, all of the yellow region comprised between the edge of the snow and the parallel of 60° north latitude, into numerous islands of small extent. Such a state of things does not cease, until the snow, reduced to its minimum area, ceases to melt. Then the breadth of the canals diminishes, the temporary sea disappears, and the yellow region again returns to its former area. The different phases of these vast phenomena are renewed at each return of the seasons, and we have been able to observe them in all their particulars very easily during the oppositions of 1882, 1884 and 1886, when the planet presented its northern pole to terrestrial spectators. The most natural and the most simple interpretation is that to which we have referred, of a great inundation produced by the melting of the snows,—it is entirely logical, and is sustained by evident analogy with terrestrial phenomena. We conclude, therefore, that the

canals are such in fact, and not only in name. The network formed by these was probably determined in its origin in the geological state of the planet, and has come to be slowly elaborated in the course of centuries. It is not necessary to suppose them the work of intelligent beings, and notwithstanding the almost geometrical appearance of all of their system, we are now inclined to believe them to be produced by the evolution of the planet, just as on the Earth we have the English Channel and the Channel of Mozambique.

It would be a problem not less curious than complicated and difficult, to study the system of this immense stream of water, upon which perhaps depends principally the organic life upon the planet, if organic life is found there. The variations of their appearance demonstrated that this system is not constant. When they become displaced, or their outlines become doubtful and ill defined, it is fair to suppose that the water is getting low, or is even entirely dried up. Then in place of the canal there remains either nothing, or at most a stripe of yellowish color differing little from the surrounding background. Sometimes they take on a nebulous appearance, for which at present it is not possible to assign a reason. At other times true enlargements are produced, expanding to 100, 200 or more kilometers [60 to 120 miles] in breadth, and this sometimes happens for canals very far from the north pole, according to laws which are unknown. This has occurred in Hydaspes in 1864, in Simois in 1879, in Ackeron in 1884, and in Triton in 1888. The diligent and minute study of the transformations of each canal may lead later to a knowledge of the cause of these facts.

The Gemination of the "Canals."—But the most surprising phenomenon pertaining to the canals of Mars is their gemination, which seems to be produced principally in the months which precede, and in those which follow the great northern inundation, at about the times of the equinoxes. In consequence of a rapid process, which certainly lasts at most a few days, or even perhaps only a few hours, and of which it has not yet been possible to determine the particulars with certainty, a given canal changes its appearance, and is found transformed through all its length, into two lines or uniform stripes, more or less parallel to one another, and which run straight and equal with the exact geometrical precision of the two rails of a railroad. But this exact course is the only point of resemblance with the rails, because in dimensions

there is no comparison possible, as it is easy to imagine. The two lines follow very nearly the direction of the original canal, and end in the place where it ended. . . .

The observation of the geminations is one of the greatest difficulty, and can only be made by an eye well practiced in such work, added to a telescope of accurate construction, and of great power. This explains why it is that it was not seen before 1882. In the ten years that have transpired since that time, it has been seen and described at eight or ten observatories. Nevertheless, some still deny that these phenomena are real, and tax with illusion (or even imposture) those who declare that they have observed it.

Life on Mars?—Having regard then to the principle that in the explanation of natural phenomena it is universally agreed to begin with the simplest suppositions, the first hypotheses on the nature and cause of the geminations have for the most part put in operation only the laws of inorganic nature. Thus, the gemination is supposed to be due either to the effects of light in the atmosphere of Mars, or to optical illusions produced by vapors in various manners, or to glacial phenomena of a perpetual winter, to which it is known all the planets will be condemned, or to double cracks in its surface, or to single cracks of which the images are doubled by the effect of smoke issuing in long lines and blown laterally by the wind. The examination of these ingenious suppositions leads us to conclude that none of them seem to correspond entirely with the observed facts, either in whole or in part. Some of these hypotheses would not have been proposed, had their authors been able to examine the geminations with their own eyes. Since some of these may ask me directly—Can you suggest anything better? I must reply candidly, No.

It would be far more easy if we were willing to introduce the forces pertaining to organic nature. Here the field of plausible supposition is immense, being capable of making an infinite number of combinations capable of satisfying the appearances even with the smallest and simplest means. Changes of vegetation over a vast area, and the production of animals, also very small, but in enormous multitudes, may well be rendered visible at such a distance. An observer placed in the Moon would be able to see such an appearance at the times in which agricultural operations are carried out upon one vast plain—the seed time and the gathering of the harvest. In such a manner also would the flowers of the plants of the great steppes of Europe and Asia be rendered visible

at the distance of Mars—by a variety of coloring. A similar system of operations produced in that planet may thus certainly be rendered visible to us. But how difficult for the Lunarians and the Areans to be able to imagine the true causes of such changes of appearance, without having first at least some superficial knowledge of terrestrial nature! So also for us, who know so little of the physical state of Mars, and nothing of its organic world, the great liberty of possible supposition renders arbitrary all explanations of this sort, and constitutes the gravest obstacle to the acquisition of well founded notions. All that we may hope is that with time the uncertainty of the problem will gradually diminish, demonstrating, if not what the geminations are, at least what they can not be. We may also confide a little in what Galileo called "the courtesy of Nature," thanks to which, sometime from an unexpected source, a ray of light will illuminate an investigation at first believed inaccessible to our speculations, and of which we have a beautiful example in celestial chemistry. Let us therefore hope and study.

LOWELL[1]

Life on Mars

(From "Mars," 1895.)

To review, now, the chain of reasoning by which we have been led to regard it probable that upon the surface of Mars we see the effects of local intelligence. We find, in the first place, that the broad physical conditions of the planet are not antagonistic to some form of life; secondly, that there is an apparent dearth of water upon the planet's surface, and therefore, if beings of sufficient intelligence inhabited it, they would have to resort to irrigation to support life; thirdly, that there turns out to be a network of markings covering the disk precisely counterparting what a system of irrigation would look like; and, lastly, that there is a set of spots placed where we should expect to find the lands thus artificially fertilized, and behaving as such constructed oases should. All this, of course, may be a set of coincidences, signifying nothing; but the probability points the other way. As to details of explanation, any we may adopt will undoubtedly be found, on closer acquaintance, to vary from the actual Martian state of things; for any Martian life must differ markedly from our own.

The fundamental fact in the matter is the dearth of water. If we keep this in mind, we shall see that many of the objections that spontaneously arise answer themselves. The supposed Herculean task of constructing such canals disappears at once; for, if the canals be dug for irrigation purposes, it is evident that what we see, and call by ellipsis the canal, is not really the canal at all, but the strip of fertilized land bordering it—the thread of water in the midst of it, the canal itself, being far too small to be perceptible. In the case of an irrigation canal seen at a distance, it is always the strip of verdure, not the canal, that is visible, as we see in looking from afar upon irrigated country on the Earth.

[1] Percival Lowell (1855–1916), American astronomer, founded the Lowell Observatory at Flagstaff, Arizona, where he and his colleagues made important visual, photographic, and spectrographic studies of planetary surfaces. Lowell is most widely known for his speculations concerning Mars.

We may, perhaps, in conclusion, consider for a moment how different in its details existence on Mars must be from existence on the Earth. One point out of many bearing on the subject, the simplest and most certain of all, is the effect of mere size of habitat upon the size of the inhabitant; for geometrical conditions alone are most potent factors in the problem of life. Volume and mass determine the force of gravity upon the surface of a planet, and this is more far-reaching in its effects than might at first be thought. Gravity on the surface of Mars is only a little more than one third what it is on the surface of the Earth. This would work in two ways to very different conditions of existence from those to which we are accustomed. To begin with, three times as much work, as for example, in digging a canal, could be done by the same expenditure of muscular force. If we were transported to Mars, we should be pleasingly surprised to find all our manual labor suddenly lightened threefold. But, indirectly, there might result a yet greater gain to our capabilities; for if Nature chose she could afford there to build her inhabitants on three times the scale she does on Earth without their ever finding it out except by interplanetary comparison . . .

It must be remembered that the question of their size has nothing to do with the question of their existence. The arguments for their presence are quite apart from any consideration of avoirdupois. No Herculean labors need to be accounted for; and, if they did, brain is far more potent to the task than brawn.

Something more we may deduce about the characteristics of possible Martians, dependent upon Mars itself, a result of the age of the world they would live in.

A planet may in a very real sense be said to have life of its own, of which what we call life may or may not be a subsequent detail. It is born, has its fiery youth, sobers into middle age, and just before this happens brings forth, if it be going to do so at all, the creatures on its surface which are, in a sense, its offspring. The speed with which it runs through its gamut of change prior to production depends upon its size; for the smaller the body the quicker it cools, and with it loss of heat means beginning of life for its offspring. It cools quicker because, as we saw in a previous chapter, it has relatively less inside for its outside, and it is through its outside that its inside cools. After it has thus become capable of bearing life, the Sun quickens that life and supports it for we know not how long. But its duration is measured at the most by

the Sun's life. Now, inasmuch as time and space are not, as some philosophers have from their too mundane standpoint supposed, forms of our intellect, but essential attributes of the universe, the time taken by any process affects the character of the process itself, as does also the size of the body undergoing it. The changes brought about in a large planet by its cooling are not, therefore, the same as those brought about in a small one. Physically, chemically, and, to our present end, organically, the two results are quite diverse. So different, indeed, are they that unless the planet have at least a certain size it will never produce what we call life, meaning our particular chain of changes or closely allied forms of it, at all. As we saw in the case of atmosphere, it will lack even the premise to such conclusion.

Whatever the particular planet's line of development, however, in its own line, it proceeds to greater and greater degrees of evolution, till the process stops, dependent, probably, upon the Sun. The point of development attained is, as regards its capabilities, measured by the planet's own age, since the one follows upon the other.

Now, in the special case of Mars, we have before us the spectacle of a world relatively well on in years, a world much older than the Earth. To so much about his age Mars bears evidence on his face. He shows unmistakable signs of being old. Advancing planetary years have left their mark legible there. His continents are all smoothed down; his oceans have all dried up. *Teres atque rotundus*, he is a steady-going body now. If once he had a chaotic youth, it has long since passed away. Although called after the most turbulent of the gods, he is at the present time, whatever he may have been once, one of the most peaceable of the heavenly host. His name is a sad misnomer; indeed, the ancients seem to have been singularly unfortunate in their choice of planetary cognomens. With Mars so peaceful, Jupiter so young, and Venus bashfully draped in cloud, the planets' names accord but ill with their temperaments.

Mars being thus old himself, we know that evolution on his surface must be similarly advanced. This only informs us of its condition relative to the planet's capabilities. Of its actual state our data are not definite enough to furnish much deduction. But from the fact that our own development has been comparatively a recent thing, and that a long time would be needed to bring even Mars to his present geological condition, we may judge any life he

may support to be not only relatively, but really older than our own.

From the little we can see, such appears to be the case. The evidence of handicraft, if such it be, points to a highly intelligent mind behind it. Irrigation, unscientifically conducted, would not give us such truly wonderful mathematical fitness in the several parts to the whole as we there behold. A mind of no mean order would seem to have presided over the system we see—a mind certainly of considerably more comprehensiveness than that which presides over the various departments of our own public works. Party politics, at all events, have had no part in them; for the system is planet wide. Quite possibly, such Martian folk are possessed of inventions of which we have not dreamed, and with them electrophones and kinetoscopes are things of a bygone past, preserved with veneration in museums as relics of the clumsy contrivances of the simple childhood of the race. Certainly what we see hints at the existence of beings who are in advance of, not behind us, in the journey of life.

Startling as the outcome of these observations may appear at first, in truth there is nothing startling about it whatever. Such possibility has been quite on the cards ever since the existence of Mars itself was recognized by the Chaldean shepherds, or whoever the still more primeval astronomers may have been. Its strangeness is a purely subjective phenomenon, arising from the instinctive reluctance of man to admit the possibility of peers. Such would be comic were it not the inevitable consequence of the constitution of the universe. To be shy of anything resembling himself is part and parcel of man's own individuality. Like the savage who fears nothing so much as a strange man, like Crusoe who grows pale at the sight of footprints not his own, the civilized thinker instinctively turns from the thought of mind other than the one he himself knows. To admit into his conception of the cosmos other finite minds as factors has in it something of the weird. Any hypothesis to explain the facts, no matter how improbable or even palpably absurd it be, is better than this. Snow-caps of solid carbonic acid gas, a planet cracked in a positively monomaniacal manner, meteors ploughing tracks across its surface with such mathematical precision that they must have been educated to the performance, and so forth and so on, in hypotheses each more astounding than its predecessor, commend themselves to man, if only by such means he may escape the admission of anything approaching his kind.

Surely all this is puerile, and should as speedily as possible be outgrown. It is simply an instinct like any other, the projection of the instinct of self-preservation. We ought, therefore, to rise above it, and, where probability points to other things, boldly accept the fact provisionally, as we should the presence of oxygen, or iron, or anything else. Let us not cheat ourselves with words. Conservatism sounds finely, and covers any amount of ignorance and fear.

We must be just as careful not to run to the other extreme, and draw deductions of purely local outgrowth. To talk of Martian beings is not to mean Martian men. Just as the probabilities point to the one, so do they point away from the other. Even on this Earth man is of the nature of an accident. He is the survival of by no means the highest physical organism. He is not even a high form of mammal. Mind has been his making. For aught we can see, some lizard or batrachian might just as well have popped into his place early in the race, and been now the dominant creature of this Earth. Under different physical conditions, he would have been certain to do so. Amid the surroundings that exist on Mars, surroundings so different from our own, we may be practically sure other organisms have been evolved of which we have no cognizance. What manner of beings they may be we lack the data even to conceive.

For answers to such problems we must look to the future. That Mars seems to be inhabited is not the last, but the first word on the subject. More important than the mere fact of existence of living beings there, is the question of what they may be like. Whether we ourselves shall live to learn this cannot, of course, be foretold. One thing, however, we can do, and that speedily: look at things from a standpoint raised above our local point of view; free our minds at least from the shackles that of necessity tether our bodies; recognize the possibility of others in the same light that we do the certainty of ourselves. That we are the sum and substance of the capabilities of the cosmos is something so preposterous as to be exquisitely comic. We pride ourselves upon being men of the world, forgetting that this is but objectionable singularity, unless we are, in some wise, men of more worlds than one. For, after all, we are but a link in a chain. Man is merely this earth's highest production up to date. That he in any sense gauges the possibilities of the universe is humorous. He does not, as we can easily foresee, even gauge those of this planet. He has

been steadily bettering from an immemorial past, and will apparently continue to improve through an incalculable future. Still less does he gauge the universe about him. He merely typifies in an imperfect way what is going on elsewhere, and what, to a mathematical certainty, is in some corners of the cosmos indefinitely excelled.

If astronomy teaches anything, it teaches that man is but a detail in the evolution of the universe, and that resemblant though diverse details are inevitably to be expected in the host of orbs around him. He learns that, though he will probably never find his double anywhere, he is destined to discover any number of cousins scattered through space.

KEELER[1]

A Spectroscopic Proof of the Meteoric Constitution of Saturn's Rings

(From *Astrophysical Journal*, Vol. 1, 1895.)

The hypothesis that the rings of Saturn are composed of an immense multitude of comparatively small bodies, revolving around Saturn in circular orbits, has been firmly established since the publication of Maxwell's classical paper in 1859. The grounds on which the hypothesis is based are too well known to require special mention. All the observed phenomena of the rings are naturally and completely explained by it, and mathematical investigation shows that a solid or fluid ring could not exist under the circumstances in which the actual ring is placed.

I have recently obtained a spectroscopic proof of the meteoric constitution of the ring, which is of interest because it is the first *direct* proof of the correctness of the accepted hypothesis, and because it illustrates in a very beautiful manner (as I think) the fruitfulness of Doppler's principle, and the value of the spectroscope as an instrument for the measurement of celestial motions.

Since the relative velocities of different parts of the ring would be essentially different under the two hypotheses of rigid structure and meteoric constitution, it is possible to distinguish between these hypotheses by measuring the motion of different parts of the ring in the line of sight. The only difficulty is to find a method so delicate that the very small differences of velocity in question may not be masked by instrumental errors. Success in visual observations of the spectrum is hardly to be expected.

Soon after the large spectroscope of the Allegheny Observatory was completed, in 1893, I attempted to determine the relative motions of different parts of the system of Saturn, by photographing the spectrum with the slit parallel to the major axis of the ring,

[1] James Edward Keeler (1857–1900), American astronomer, director of the Allegheny Observatory and later of the Lick Observatory, is best known for his photographic and spectroscopic work on nebulæ and for his study of Saturn's rings.

but failed to obtain satisfactory results. The unfavorable atmospheric conditions at Allegheny, the strong yellow color of the objective of the thirteen-inch equatorial, and the yellow color of Saturn itself so reduced the intensity of the violet part of the spectrum that the negatives obtained with a sufficiently high dispersion were too weak and granular to admit of measurement. Another unfavorable circumstance was the fact that I had to guide the practically invisible image corresponding to the $H\gamma$ line by means of the visual image, which was greatly out of focus on account of the chromatic aberration of the visually corrected telescope. Having recently obtained excellent results in other directions with orthochromatic plates, by the use of which the difficulties mentioned above are to a great extent obviated, I was induced to repeat my earlier attempts, and obtained two fine photographs of the lower spectrum of Saturn on April 9 and 10 of the present year. The exposure in each case was two hours, and the image of the planet was kept very accurately central on the slit-plate. After the exposure the spectrum of the Moon was photographed on each side of the spectrum of Saturn, and nearly in contact with it. Each part of the lunar spectrum has a width of about one millimeter, which is also nearly the extreme width of the planetary spectrum. On both sides of the spectrum of the ball of the planet are the narrow spectra of the ansæ of the ring.

The spectrum of Saturn, showing the meteoric composition of the ring. a. Comparison spectrum of iron. b. Spectrum of one side of ring. c. Spectrum of ball of planet. d. Spectrum of other side of ring. e. Iron spectrum. f. Wave length in Ångstroms. *Lowell Observatory.*

The length of the spectrum from *b* to *D* is 23 millimeters. The focus was adjusted on the line λ5352, a little above the position of maximum sensitiveness of an orthochromatic plate, in the yellow green. On both plates the densities of the different spectra are very nearly equal, and the definition is excellent. It is hardly necessary to say that all the lenses used in the apparatus are visually corrected objectives.

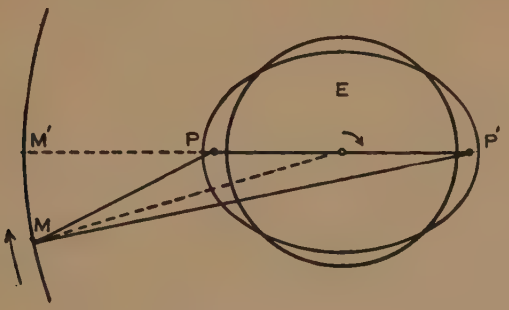
These photographs not only show very clearly the relative displacement of the lines in the spectrum of the ring, due to the opposite motions of the ansæ, but exhibit another peculiarity, which is of special importance in connection with the subject of the present paper. The planetary lines are strongly inclined, in consequence of the rotation of the ball, but the lines in the spectra of the ansæ do not follow the direction of the lines in the central spectrum; they are nearly parallel to the lines of the comparison spectrum, and, in fact, as compared with the lines of the ball, have a slight tendency to incline in the opposite direction. Hence the outer ends of these lines are less displaced than the inner ends. Now it is evident that if the ring rotated as a whole the velocity of the outer edge would exceed that of the inner edge, and the lines of the ansæ would be inclined in the same direction as those of the ball of the planet. If, on the other hand, the ring is an aggregation of satellites revolving around Saturn, the velocity would be greatest at the inner edge, and the inclination of lines in the spectra of the ansæ would be reversed. The photographs are, therefore, a direct proof of the approximate correctness of the latter supposition.

G. H. DARWIN[1]

On the Origin of the Moon

(From "The Evolution of Satellites," *Annual Report* of the Smithsonian Institution for 1897.)

Now let us apply these ideas to the case of the earth and the moon. A man standing on the planet, as it rotates, is carried past places where the fluid is deeper and shallower alternately; at the deep places he says that it is high tide, and at the shallow places that it is low tide. In the figure it is high tide when the observer is carried past *P*. Now, it was pointed out that when there is no fluid friction we must put the moon at *M'*, but when there is friction



she must be at *M*. Accordingly, if there is no friction it is high tide when the moon is over the observer's head, but when there is fluid friction the moon has passed his zenith before he reaches high tide. Hence he would remark that fluid friction retards the time of high water.

A day is the name for the time in which the earth rotates once, and a month for the time in which the moon revolves once. Then,

[1] George Howard Darwin (1845–1912), English mathematical astronomer, son of the great biologist Charles Darwin, commenced his epochal researches on tidal friction in 1879. His book "The Tides" published in 1898 is a summary of his investigations and gives a theory of the evolution of celestial systems.

since tidal friction retards the earth's rotation and the moon's revolution, we may state that both the day and the month are being lengthened, and that these results follow from the retardation in the time of high tide. It must also be noted that the spiral in which the moon moves is an increasing one, so that her distance from the earth increases. These are absolutely certain and inevitable results of the mechanical interaction of the two bodies.

At the present time the rates of increase of the day and month are excessively small, so that it has not been found possible to determine them with any approach to accuracy. It may be well to notice in passing that if the rate of change of either element were determinable, that of the other would be deducible by calculation.

The extreme slowness of the changes within historical times is established by the records in early Greek and Assyrian history of eclipses of the sun which occurred on certain days and at certain places. Notwithstanding the changes in the calendar, it is possible to identify the day according to our modern reckoning, and the identification of the place presents no difficulty. Astronomy affords the means of calculating the exact time and place of the occurrence of an eclipse even three thousand years ago, on the supposition that the earth spun at the same rate then as now, and that the complex laws governing the moon's motion are unchanged. The particular eclipse referred to in history is known, but any considerable change in the earth's rotation and in the moon's motion would have shifted the position of visibility on the earth from the situation to which modern computation would assign it. Most astronomical observations would be worthless if the exact time of the occurrence were uncertain, but in the case of eclipses the place of observation affords just that element of precision which is otherwise wanting. As, then, the situations of the ancient eclipses agree fairly well with modern computations, we are sure that there has been no great change within the last three thousand years either in the earth's rotation or in the moon's motion. There is, however, a small outstanding discrepancy which indicates that there has been some change. But the exact amount involves elements of uncertainty, because our knowledge of the laws of the moon's motion is not yet quite accurate enough for the absolutely perfect calculation of eclipses which occurred many centuries ago. In this way it is known that within historical times the retardation of the earth's rotation and the recession of the moon have been, at any rate, very slight.

It does not follow from this that the changes have always been equally slow, and indeed it may be shown by mathematical arguments that the efficiency of tidal friction increases with enormous rapidity as we bring the tide-raising satellite nearer to the planet. The law of tidal friction is that it varies according to the inverse sixth power of the distance; so that with the moon at half her present distance, the rate of retardation of the earth's rotation would be sixty-four times as great as it now is. Thus, although the action may now be almost insensibly slow, yet it must have proceeded with much greater rapidity when the moon was nearer to us.

There are many problems in which it would be very difficult to follow the changes in the system according to the times of their occurrence, but where it is possible to banish time, and to trace the changes themselves in due order, without reference to time. In the sphere of common life, we know the succession of stations which a train must pass between New York and Boston, although we may have no time-table. This is the case with our astronomical problem; for although we have no time-table, yet the sequence of the changes in the system may be traced accurately.

Let us then banish time, and look forward to the ultimate outcome of the tidal interaction of the moon and the earth. The day and the month are now lengthening at relative rates which are calculable, although the absolute rates in time are unknown. It will suffice for a general comprehension of the problem to know that the present rate of increase of the day is much more rapid than that of the month, and that this will hold good in the future. Thus, the number of rotations of the earth in the interval comprised in one revolution of the moon diminishes; or, in other words, the number of days in the month diminishes, although the length of each day increases so rapidly that the month itself is longer than at present. For example, when the day shall be equal in length to two of our actual days, the month may be as long as thirty-seven of our days, and then the earth will spin round only about eighteen times in the month.

This gradual change in the day and the month proceeds continuously until the duration of a rotation of the earth is prolonged to fifty-five of our present days. At the same time, the month, or the time of a revolution of the moon around the earth, will also occupy fifty-five of our days. Since the month here means the period of the return of the moon to the same place amongst the

stars, and since the day is to be estimated in the same way, the moon must then always face the same part of the earth's surface, and the two bodies must move as though they were united by a bar. The outcome of the lunar tidal friction will, therefore, be that the moon and the earth will go round as though locked together in a period of fifty-five of our present days, with day and month identical in length.

Now, looking backward in time, we find the day and the month shortening, but the day changing more rapidly than the month. The earth was therefore able to complete more revolutions in the month, although that month was itself shorter than it is now. We get back, in fact, to a time when there were twenty-nine rotations of the earth in the time of the moon's revolution, instead of twenty-seven and one-third, as at present. This epoch is a sort of crisis in the history of the moon and the earth, for it may be proved that there never could have been more than twenty-nine days in the month. Earlier than this epoch, the days were fewer than twenty-nine; and later, fewer also. Although measured in years, this epoch in the earth's history must be very remote; yet when we contemplate the whole series of changes it must be considered as a comparatively recent event. In a sense, indeed, we may be said to have passed recently through the middle stage of our history.

Now, pursuing the series of changes further back than the epoch when there was the maximum number of days in the month, we find the earth still rotating faster and faster, and the moon drawing nearer and nearer to the earth and revolving in shorter and shorter periods. But a change has supervened, so that the rate at which the month is shortening is more rapid than the rate of change in the day. Consequently, the moon now gains, as it were, on the earth, which can not get round so frequently in the month as it did before. In other words, the number of days in the month declines from the maximum of twenty-nine, and is finally reduced to one. When there is only one day in the month the earth and the moon go round at the same rate, so that the moon always looks at the same side of the earth, and as far as concerns the motion they might be fastened together by iron bands.

This is the same conclusion at which we arrived with respect to the remote future. But the two cases differ widely; for whereas, in the future, the period of the common rotation will be fifty-five of our present days, in the past we find the two bodies going round

each other in between three and five of our present hours. A satellite revolving round the earth in so short a period must almost touch the earth's surface. The system is therefore, traced until the moon nearly touches the earth, and the two go round each other like a single solid body in about three to five hours.

The series of changes has been traced forward and backward from the present time, but it will make the whole process more intelligible, and the opportunity will be afforded for certain further considerations, if I sketch the history again in the form of a continuous narrative.

Let us imagine a planet attended by a satellite which revolves in a circular orbit so as nearly to touch its surface, and continuously to face the same side of the planet. If now, for some cause, the satellite's month comes to differ very slightly from the planet's day, the satellite will no longer continuously face the same side of the planet, but will pass over every part of the planet's equator in turn. This is the condition necessary for the generation of tidal oscillations in the planet, and as the molten lava, of which we suppose the planet to be formed, is a sticky or viscous fluid, the tides must be subject to friction. Tidal friction will then begin to do its work, but the result will be very different according as the satellite revolves a little faster or a little slower than the planet. If it revolves a little faster, so that the month is shorter than the day, we have a condition not contemplated in the figure above; it is easy to see, however, that as the satellite is always leaving the planet behind it, the apex of the tidal protuberance must be directed to a point behind the satellite in its orbit. In this case, the rotation of the planet must be accelerated by the tidal friction, and the satellite must be drawn inward toward the planet, into which it must ultimately fall. In the application of this theory to the earth and the moon, it is obvious that the very existence of the moon negatives the hypothesis that the initial month was even infinitesimally shorter than the day. We must then suppose that the moon revolved a little more slowly than the earth rotated. In this case, the tidal friction would retard the earth's rotation, and force the moon to recede from the earth, and so perform her orbit more slowly. Accordingly, the primitive day and the primitive month lengthen, but the month increases much more rapidly than the day, so that the number of days in the month becomes greater. This proceeds until that number reaches a maximum, which in the case of our planet is about twenty-nine.

After the epoch of maximum number of days in the month, the rate of change in the length of the day becomes less[1] rapid than that in the length of the month; and although both periods increase, the number of days in the month begins to diminish. The series of changes then proceeds until the two periods come again to an identity, when we have the earth and the moon, as they were at the beginning, revolving in the same period, with the moon always facing the same side of the planet. But in her final condition the moon will be a long way off from the earth, instead of being quite close to it.

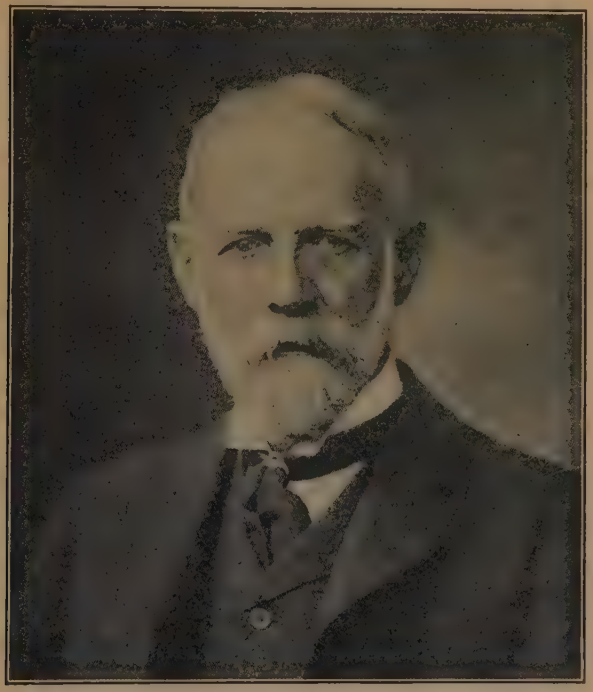
Although the initial and final states resemble each other, yet they differ in one respect which is of much importance; for in the initial condition the motion is unstable, whilst finally it is stable. The meaning of this is that if the moon were even infinitesimally disturbed from the initial mode of motion, she would necessarily either fall into the planet or recede thereform, and it would be impossible for her to continue to move in that neighborhood. She is unstable in the same sense in which an egg balanced on its point is unstable; the smallest mote of dust will upset it, and practically it can not stay in that position. But the final condition resembles the case of an egg lying on its side, which only rocks a little when we disturb it. So if the moon were slightly disturbed from her final condition, she would continue to describe very nearly the same path round the earth, and would not assume some entirely new form of orbit.

It is by methods of rigorous argument that the moon is traced back to the initial unstable condition when she revolved close to the earth. But the argument here breaks down, and calculation is incompetent to tell us what occurred before, and how she attained that unstable mode of motion. We can only speculate as to the preceding history, but there is some basis for our speculation; for I say that if a planet, such as the earth, made each rotation in a period of three hours, it would very nearly fly to pieces. The attraction of gravity would be barely strong enough to hold it together, just as the cohesive strength of iron is insufficient to hold a fly-wheel together if it is spun too fast. There is, of course, an important distinction between the case of the ruptured fly-wheel and the supposed break-up of the earth; for when the fly-wheel breaks, the pieces are hurled apart as soon as the force of cohesion fails, whereas when a planet breaks up through too rapid rotation,

[1 Presumably this should read "more rapid."]

gravity must continue to hold the pieces together after they have ceased to form parts of a single body.

Hence we have grounds for conjecturing that the moon is composed of fragments of the primitive planet which we now call the earth, which detached themselves when the planet spun very swiftly, and afterwards became consolidated. It surpasses the powers of mathematical calculation to trace the details of the



Sir George Darwin.

process of this rupture and subsequent consolidation, but we can hardly doubt that the system would pass through a period of turbulence before order was reestablished in the formation of a satellite.

I have said that rapid rotation was probably the cause of the birth of the moon, but this statement needs qualification. There are certain considerations which prevent us from ascertaining the common period of revolution of the moon and the earth with accuracy; it may lie between three and five hours. I think that such a speed might not, perhaps, be quite sufficient to cause the

planet to break up. Is it possible, then, to suggest any other cause which might have cooperated with the tendency to instability of the rotating planet? I think that there is such a cause; and though we are here dealing with guesswork, I will hazard the suggestion.

The primitive planet, before the birth of the moon, was rotating rapidly with reference to the sun, and it must, therefore, have been agitated by tidal oscillations due to the sun's attraction. Now, the magnitude of these solar tides is much influenced by the speed of rotation of the planet, and mathematical reasoning appears to show that when the day was about three or four hours in length the oscillations must have been very great, although the sun stood no nearer to the earth then than it does now. May we not conjecture that the oscillation of the molten planet became so violent that, in cooperation with the rapid rotation, it shook the planet to pieces, detaching huge fragments which ultimately were consolidated into the moon? There is nothing to tell us whether this theory affords the true explanation of the birth of the moon, and I say that it is only a wild speculation, incapable of verification.

But the truth or falsity of this speculation does not militate against the acceptance of the general theory of tidal friction, which, standing on the firm basis of mechanical necessity, throws much light on the history of the earth and the moon, and correlates the lengths of our present day and month.

I have said above that the sequence of events has been stated without reference to the scale of time. It is of the utmost importance, however, to gain some idea of the time requisite for all the changes in the system. If millions of millions of years were necessary, the application of the theory to the moon and the earth would have to be rejected, because it is known from other lines of argument that there is not an unlimited bank of time on which to draw. The uncertainty as to the duration of the solar system is wide, yet we are sure that it has not existed for an almost infinite past.

Now, although the actual time-scale is indeterminate, it is possible to find the minimum time adequate for the transformation of the moon's orbit from its supposed initial condition to its present shape. It may be proved, in fact, that if tidal friction had always operated under the conditions most favorable for producing rapid change, the sequence of events from the beginning until today would have occupied a period of between fifty and sixty millions of

years. The actual period, of course, must have been much greater. Various lines of argument as to the age of the solar system have led to results which differ widely among themselves, yet I can not think that the applicability of the theory of tidal friction is negatived by the magnitude of the period demanded. It may be that science will have to reject the theory in its full extent, but it seems improbable that the ultimate verdict will be adverse to the preponderating influence of the tide on the evolution of our planet.

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